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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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TEACHER-PUPIL ATTITUDES AND CLAIRVOYANCE TEST RESULTS

By MARGARET ANDERSON AND RHEA WHITE[1]

ABSTRACT: A clairvoyance experiment was conducted in the classroom in seven high schools in five different states of the U.S.A. The teacher administered the tests. The objective was to discover whether any effect on scoring rate in the test might be associated with the attitude of the teacher toward the individual pupil, the liking of the pupil for the teacher, or the combination of the two. There were 28,500 trials by 228 subjects in classes ranging over a number of high school courses. Each subject was given a sealed envelope containing a list of ESP symbols. He was asked to write down, on an outside record sheet, what he thought those symbols were. The record sheets and the unopened envelopes were returned to the authors for checking. Teachers and pupils also filled out questionnaires designed to reveal their attitudes toward each other.

The overall results of the clairvoyance tests were insignificant. However, when the ESP results were broken down according to the teacher's attitude toward the student, significantly positive scoring was associated with a positive attitude, insignificant negative scoring was associated with a negative attitude, and there was a significant difference between the two. When the total results were divided according to whether the student was highly positive, moderately positive, or negative in his attitude toward the teacher, the first group gave significant positive results; the second, chance results; and the third, significant negative results. Thus negative attitudes gave negative ESP scoring for both teachers and students.

When the teacher and pupil attitudes were combined, it was found that positive attitudes on the part of teacher and pupil toward each other gave ESP results with a highly significant positive deviation and the combination of negative attitudes gave results with a significant negative deviation.

Other important relations are indicated, the most important being the measure of consistency of ESP scoring in relation to attitudes. A chi-square test of consistency applied to the combination of attitudes on the part of teacher and pupil gave a highly significant result. It is important to recognize that these very significant and consistent relations between teacher and pupil as associated with ESP scores were derived from clairvoyance tests, not from tests in which the teacher participated as agent. Likewise, the point is made that it was not the amount of ESP but the direction of scoring, positive or negative, that varied with the teacher-pupil relation.—Ed.

[1] The authors were partly supported in the work reported here by grants from the Parapsychology Foundation, Inc., of New York City, and both hold Ralph Drake Perry Fellowships in the Parapsychology Laboratory of Duke University. They wish to express their appreciation for the cooperation given by the seven teachers and their respective students.

Introduction

It is well known from research reported in the literature of parapsychology that the sender-receiver relationship in GESP tests is important. MacFarland's (**6**) experiment with two agents working simultaneously with the same subjects, but individual target orders, revealed that the subjects tended to score differently with different experimenters. Stuart (**12**), working with drawings, found that couples who had a close relationship (twins, engaged and married couples) gave significantly positive results, while unrelated pairs gave significantly negative results. Casper (**3**), working from the hypothesis that the closer the emotional relationship between subject and agent in GESP tests, the higher would be the scores, found that the positive scoring was associated with the least-liked senders, while the lowest scores were obtained with the best-liked senders. This difference was significant ($P=.001$). He found no important difference when clairvoyance tests in the same situation were used. Soal and Goldney (**11**), working with Basil Shackleton, discovered that this subject was able to score significantly in GESP tests with only three out of the eleven agents tried. Soal and Bateman (**10**), working with Mrs. Gloria Stewart as subject, noted a similar discrimination among agents. It should also be mentioned in this connection that van Busschbach, in his GESP experiments with school children (**13, 14, 15**), found that his subjects were able to produce significant results only when the agent was their teacher with whom they were thoroughly familiar.

In clairvoyance tests the experimenter-subject relation is also important. Sharp and Clark (**8**) observed that the attitude and actions of the experimenter appeared to some extent to be reflected in the scores of the subjects. A joint experiment was conducted by Pratt and Price (**7**) to determine whether a difference in the approach of the experimenters was accompanied by differences in scoring. The results suggested that failure to find evidence of ESP in card tests may be due to an unfavorable experimenter-subject relationship. In regard to this question of the effect of the experimenter upon subjects' scoring in clairvoyance tests, it also might be worth while to mention the failure of Soal to obtain results in clairvoyance tests—even with the Spanish girl Maria (**5**) who had scored high in clairvoyance tests with the Langdon-Davies as experi-

menters—although he has obtained highly significant results with subjects doing GESP tests.

The first attempt to have the experimenter and subjects rate each other was reported in 1950 by Woodruff and Dale (**16**). Each one of these investigators independently tested the same subjects for clairvoyance. The subjects answered a point-scale questionnaire regarding their attitude toward the experimenter, the test, and ESP in general. Four of the items, each receiving a rating of from -2 to $+2$, were concerned just with the experimenter. All the points given for these four questions were totalled and the mean was obtained. The ratings which fell above the mean were considered "high," and those that fell below the mean were called "low" ratings. In the case of Dale, the separation based on these ratings was negligible. But where Woodruff was the experimenter, the ESP scores for the subjects who gave him low ratings were positive, and the scores associated with high ratings were negative. The CR of the difference was significant ($P=.0034$).

For the purpose of conducting the further study of subject-experimenter relations reported here, an area was needed that offered large numbers of subjects having the same experimenter, with the subjects being tested under similar conditions. The typical public school classroom provided such a testing situation, and earlier work indicated that ESP may be demonstrated by school children in their normal setting. Successful results under these conditions had already been obtained by Bond (**1**). The extensive work recently done by van Busschbach (**13, 14, 15**) indicates that the relationship already established between teacher and pupil favors success in ESP performance, at least under GESP test conditions.

The authors designed their experiment with this background of research in mind, although for simplification of control they selected a clairvoyance test using sealed enclosed targets. The work was carried out in the spring of 1956. It was directed primarily at three questions: To what extent is the rate of ESP scoring related to (1) the teacher's feeling for the student? (2) the student's feeling for the teacher? and (3) the combination of the teacher's and student's feeling for each other?

Procedure

Eight academic high school teachers, four men and four women, in five states, known directly or indirectly to the experimenters, were

asked to cooperate in the experiment. When the request was made, the teachers were told that they would have to: (1) distribute the student test envelopes; (2) allow their students to answer a questionnaire concerning the teacher (a copy of the questionnaire was sent to the teacher at this time); and (3) indicate their attitude toward each student participating in the test. All eight teachers promptly replied that they would willingly cooperate. Two of the teachers were familiar with parapsychology prior to the experiment. The other six were told briefly, by letter, about the research project. Four of these six accepted the possibility of ESP; two were uncertain but nevertheless wanted to take part. The results of the two teachers who were doubtful were not different from the overall results for the whole experiment.

The teachers were asked to select a class of their choice for the experiment. Each teacher received a standard sheet of instructions[2] with which he was asked to familiarize himself before administering the test. When the test was given, the teachers were asked to "tell," not "read," it to the class. As the high school student, theoretically at least, regards his teacher in the light of the educational nature of their relationship, the test was also presented to the subjects from an intellectual basis. For one thing, they were given a brief explanation of the mathematics involved. No reward was offered. Instead, for motivational purposes, the teacher expressed his request and desire that the student do his best both for the student's own self, but even more, for the teacher.

At the beginning of the test, each student received an envelope containing a standard ESP record sheet on which were written five runs of ESP symbols, twenty-five trials to each run. These target sheets had been made up at the Parapsychology Laboratory on the basis of *"Tables of Random Sampling Numbers,"* by M. G. Kendall and B. Babington Smith, 1954. A different target order was used for each sheet; the sheets were numbered, and a carbon copy of each was kept on file at the Parapsychology Laboratory. The record sheets were placed between three sheets of opaque paper—two on top and one underneath—and enclosed in a manila envelope, which was stapled shut. Two ESP record sheets were stapled to the outside of the envelope, thus serving as an additional means of sealing the envelope. There was a piece of carbon paper between these two sheets so that a copy of each subject's ESP calls could be obtained.

[2] See Appendix A: "Letter of Instruction to Teachers."

The carbon copies were used for an independent recheck after the experiment was completed.

Each student was asked to write on the topmost outer record sheet the symbols which he thought were written on the target record sheet enclosed in the envelope. The students were allowed to make their calls in their own way and at their own rate of speed.

A small white envelope was stapled on the back of the manila test envelope. It contained a questionnaire regarding the student's opinion of the teacher.[3] After the ESP test was completed, the teacher asked the students to answer the questionnaires and then to replace them in the white envelopes and seal them. He emphasized that the students should answer honestly and assured them that he would not see their answers.

Finally, the teacher completed a statement of his own regarding his acceptance or rejection of each of the students in his class. The question, to which a *yes* or *no* answer was requested, was: "If you could form your ideal group for this class, would you include this student?" It was believed that a question stated in such a manner, while it did not directly ask whether or not the teacher liked the student, would nevertheless get at this indirectly by cutting through the various factors constituting a teacher's attitude toward a student.

When the tests had been completed, the teacher mailed all of the material, unopened, to the senior author (M.A.). The experimenters together removed each questionnaire from the white outside envelope and copied on it the name of the student, which had been written on the topmost outer record sheet. (In order to encourage frank answers, the students had not been requested to put their names on the questionnaires. In no case did either a teacher or a student object to the questions on this aspect of the test.) The names were transferred to the questionnaires at this time during the check-up in order to avoid confusion later on. The questionnaires were then put aside and were not rated until the ESP scores had been checked.

Next, the ESP envelopes were unsealed, unstapled, and opened—again, always in the presence of both experimenters. The name which each student had written on the outer record sheet was then written on the inner target record sheet. Conversely, the number found on the inner sheet was put on the outer sheet, as well as on the carbon copy of the calls.

[3] See Appendix B: "Student Questionnaire."

With both experimenters participating, the symbols from the inner target sheet were then checked against the calls written on the carbon copies of the outside record sheets. When this was finished, the student questionnaires were rated. Before the ESP test material was checked, each possible answer to the attitude questionnaire had been assigned a numerical rating. It had been decided that a total rating from 10-9 for the entire questionnaire would be indicative of a very positive or good feeling, 8-7 would be considered average to good, while 6-0 would be regarded as negative or poor feeling. Thus tabulations were made for each student on the basis of his ESP score, acceptance or rejection of him by his teacher, and his own classification of the teacher into one of the three possible categories.

In the case of five out of the seven classes the full program of checking was carried through to completion with the joint collaboration of both experimenters. In the case of the other two, all of the checking was done jointly except for the rating of the questionnaires, which was done first by M.A. and later checked by R.W.

A separate checkup was then made by Mrs. John Randall of the ESP tests as well as the ratings on the questionnaires and the classification of each pupil by the teacher. The complete recheck was done without knowledge of the ESP scores or attitude ratings found on the initial check-up.

Results

One of the scheduled teachers was unable to take part in the experiment because a fire destroyed the high school in which she was teaching. The data, therefore, concern seven secondary school teachers in the field of English, Spanish, speech, chemistry, physics, and trigonometry and 228 subjects (distributed in grades 9-12) doing a total of 1,140 runs or 28,500 calls The overall deviation is +78, CR=1.15.

The extent to which ESP scoring is related to the direction of the teacher's feeling is shown in Table 1. Where the teacher is positive in feeling toward 132 subjects, there is a significantly positive CR of 2.80. The 96 subjects to whom the teacher is negative in feeling give a negative CR of 1.51, which, while not significant, is suggestive. The CR of the difference between the two groups is 3.0 (P=.003).

Table 1

ESP Results as Related to Teacher's Attitude Toward Student

Teacher's Attitude Toward Student	No of Subjects	No. of Runs	Av. Run Score	Dev.	CR	P
Positive.....	132	660	5.22	+144	2.80	.005
Negative.....	96	480	4.86	− 66	1.51	.13
Total.....	228	1,140	5.07	+ 78	1.15	.25

CR_d (Pos. attitude − neg. attitude) = 3.00
P = .003

In order to evaluate the consistency of the scoring obtained when the teacher was positive or negative in feeling, the results were arranged in a two-by-two contingency table. The number of subjects whose total score was above mean chance expectation (M.C.E.), and the number of subjects whose scores were at or below M.C.E., divided according to the teacher's feelings, are given in Table 2.

Table 2

Student ESP Score Level in Relation to Teacher's Attitude Toward Student

	Teacher Positive	Teacher Negative	Total
Students With ESP Scores Above M. C. E.	70	40	110
Students With ESP Scores at or Below M. C. E.	62	56	118
Total	132	96	228

$\chi^2 = 2.85$ (1 d.f.)
P = .09

The results of this analysis of individual scoring trends in relation to teacher attitudes are not significant (P=.09).

The direction of the student's feeling for the teacher appears to be highly related to his rate of scoring, particularly at the highest level (10-9) and the lowest level (6-0). Table 3 indicates that 103 subjects, reacting to the teacher at the highest level, gave a positive CR of 3.92. At the other end, 59 subjects, with a negative attitude

toward the teacher, present a negative CR of 3.78. The CR of the difference between these two groups is 5.41. The 66 subjects who are in the average-to-positive classification (8-7 level) of feeling for the teacher give a positive CR of .83.

Table 3

ESP Results for the Three Levels of Student's Attitude Toward Teacher

Rating of Teacher by Student	No. of Subjects	No. of Runs	Av. Run Score	Dev.	CR	P
10 - 9 (Positive)	103	515	5.35	+178	3.92	.000,09
8 - 7 (Positive)	66	330	5.09	+ 30	.83	.41
Total Positive Ratings	169	845	5.25	+208	3.58	.000,3
6 - 0 (Negative)	59	295	4.56	−130	3.78	.000,15

CR_d (Total pos. rating − Total neg. rating) = 5.07
P = .000,000,6

Combining all of those who are average-to-positive in feeling, the 10-7 level, 169 subjects give a positive CR of 3.58. The CR between these positive 169 subjects and the 59 negative reacting students is 5.07.

A chi-square test was made of the distribution of ESP hits and

Table 4

Chi-Square Test of Distribution of ESP Scores in Relation to Student's Attitude Toward Teacher

ESP Results	Student's Rating of Teacher			Total
	10 - 9 Level	8 - 7 Level	6 - 0 Level	
Observed Hits	2753	1680	1345	5,778
Expected Hits	2610	1673	1495	
Observed Misses	10122	6570	6030	22,722
Expected Misses	10265	6577	5880	
Total	12,875	8,250	7,375	28,500

$\chi^2 = 28.76$ (2 d.f.)
P < .000,01

misses in relation to the three levels of student's attitude toward the teacher. The chi-square of the three-by-two table is 28.76 (2 d.f.) with P<.000,01. (See Table 4.)

In order to determine the consistency of the scoring obtained when the student was positive or negative in feeling, the results were arranged in a two-by-two contingency table. (See Table 5.) All subjects whose ratings were positive (both in 10-9 and 8-7 levels) are included in the positive group in the table. The chi-square is 13.09 (1 d.f.) with P=.000,2.

Table 5

STUDENT ESP SCORE LEVEL IN RELATION TO STUDENT'S ATTITUDE TOWARD TEACHER

	Students With Positive Attitude	Students With Negative Attitude	Total
Students With ESP Scores Above M. C. E.	92	16	108
Students With ESP Scores at or Below M. C. E.	77	43	120
Total	169	59	228

$\chi^2 = 13.09$ (1 d.f.)
P= .000,2

The results of the ESP scoring when the direction of the teacher's and the student's feeling is the same are given in Table 6. Seventy-three students who rated their teacher 10-9 were also given positive ratings by these teachers. The CR of their scores is a significantly positive 5.08 (P=.000,000,3). At the lower end of the scale, 37 subjects, experiencing a mutually unfavorable relationship with the teacher, scored a significantly negative CR of 2.50 (P=.01). The CR of the difference between these two conditions is 5.00 (P=.000,000,6).

Combining the highly positive (10-9 level) and the average-to-positive (8-7 level) students when the teacher is positive gives a CR of 4.39 (P=.000,01). The CR of the difference between this group and the mutually negative group is 4.36 (P=<.000,001). As with the teachers as a group and the students as a group, a two-by-two contingency table was made to ascertain the consistency of

Table 6

ESP Results as Related to Attitude of Teacher and Student Toward Each Other

Rating of Teacher by Student	Teacher's Attitude Positive					Teacher's Attitude Negative				
	No. of Subjects	No. of Runs	Av. Run Score	CR	P	No. of Subjects	No. of Runs	Av. Run Score	CR	P
10 - 9 (Positive)	73	365	5.53	5.08	.000,000,3	30	150	4.89	....	.....
8 - 7 (Positive)	37	185	5.06		.....	29	145	5.12	....	.....
Total Positive Ratings	110	550	5.37	4.39	.000,01	59	295	5.01	....	.....
6 - 0 (Negative)	22	110	4.44	2.96	.003	37	185	4.63	2.50	.01

CR_d (Total pos. rating − Tot. neg. rating) = 4.36
P < .000,001

scoring when the teacher and the student were mutually positive and when the teacher and the student were mutually negative. (See Table 7.) The resulting chi-square is 7.89 with P=.005.

Table 7

Student ESP Score Level When Student and Teacher Had Same Attitude Toward Each Other

	Teacher and Student Positive	Teacher and Student Negative	Total
Students With ESP Scores Above M. C. E.	65	12	77
Students With ESP Scores at or Below M. C. E.	45	25	70
Total	110	37	147

$\chi^2 = 7.89$ (1 d.f.)
P < .005

A product-moment correlation of the subjects' scoring with their questionnaire ratings is $r = +.37$ which for $N = 228$ is highly

significant (P=.000,000,002). The correlation of subjects' scoring and their questionnaire rating when the teacher is positive, based on 132 cases, is $r=+.45$ (P=.000,000,01); when the teacher is negative, in 96 cases, the correlation is $r=+.23$ (P=.022). These results are not offered as the main support of any conclusion mentioned in this paper because the scores on the student attitude questionnaire were not randomly distributed as required for the application of a product-moment correlation.

Discussion

The results of this experiment may be said to add further evidence for the case of clairvoyance. Also, the findings indicate a significant relation between the rate of ESP scoring and the attitudes registered, as well as the combination of these attitudes.

When interpreting the results in the light of the student evaluations of the teacher, the question may be raised as to whether information gained by this method is trustworthy. In *Teachers and Teaching,* Frank Hart (4) presented a study designed to ascertain whether student evaluations were reliable. Testing over ten thousand high school students and a proportionate number of teachers, he established that pupil evaluation is reliable.

Various researches on pupil-rating scales have been done by R. C. Bryan, R. M. Symonds, T. L. Torgerson, and Stuart Tiedeman, to mention but a few. The Minnesota Teacher Attitude Inventory (MTAI), a recent development, is designed to measure those attitudes of a teacher which predict how well he will get along with pupils in interpersonal relationships. While the MTAI is a more convenient instrument to use generally, it could not be used in this experiment since each individual student's reaction was desired.

Use of student evaluations is an accepted practice. It is pertinent to point out also that in a ten-year program report published in 1954 by Robert Bush (2) of a case study of teacher-pupil relationships, it is concluded that the teacher's liking for a student is positively related to all characteristics on which the teacher rates the student, such as conduct in class, intellectual qualities, achievement in the subject, quality of thinking, etc. Likewise, the student's liking for the teacher is positively related to all of the characteristics on which the student rates the teacher, such as discipline ability, knowledge of the subject, understanding, fairness, etc.

In addition to the results cited, there are other interesting observations which may provoke thought concerning the testing situation as such. Although in this test the teacher did not act as an agent, his role did have similarities to that of an acting experimenter in that he gave the instructions for the performance of the test and encouraged the subject to do well. There is some indication in the results that the level of ESP scoring is more closely related to the subject's feeling for the teacher than the latter's feeling for the subject. This is noticeable in the results of the students who are negative in their feeling. Table 6 indicates that when the subject is negative, he seemingly scores negatively regardless of the teacher's feeling for him. One would suspect from this that students with a positive attitude would score positively regardless of the teacher's feeling. This, as the table reveals, does not necessarily follow. As a curious aside, Bush concluded in his teacher-pupil relationship report that "the facts do not support the common view that a teacher's personal liking for pupils is more important than the pupil's liking for the teacher. The personal liking of a pupil for his teacher is one of the most powerful factors in bringing about an effective learning situation."

The fact that clairvoyance was used throughout this experiment is a matter that should be emphasized. It was already evident from the work done by Bond and van Busschbach that classroom testing provided a good situation for GESP results. The present experiment shows that the classroom setting also makes it possible for clairvoyance to be demonstrated. This brings us to the question of the effect of the teacher-pupil relation on the student's scoring. In van Busschbach's work, significant results were obtained only when the regular teacher acted as agent in the GESP experiment. The work suggests that the efficiency of the ESP process itself was affected by the attitudes of the participants, and it was easy to suppose that the results were linked with the role of the agent. But in the present clairvoyance experiment, the teacher could be considered as being merely part of the "environment." She distributed the test materials and gave the directions to the students but was not actively essential to the ESP operation in the test. And yet, as evidenced by the results reported here, the teacher-pupil attitudes greatly affected the scoring level of the students. It will be a question for further research to decide whether the influence of the teacher is an outgrowth of rapport established over a long period of association with the student or

whether the test results are the outcome of the particular attitude of the moment.

Another important aspect of the results is found in the significant negative deviation accompanying the combined negative attitudes. This result shows that the effect measured by the teacher-pupil questionnaires is associated with the direction of the deviation rather than the amount of ESP registered in the tests. It indicates that ESP is present but is demonstrated in a different way—as an avoidance reaction—in association with these negative attitudes of teacher and pupil. This tendency to avoid the target so as to produce a negative deviation is already well established. It has been called "psi-missing" in an article written by J. B. Rhine (8) in 1952 in which all experimental work dealing with this effect up until that time was summarized. The present experiment is another clear-cut example of this tendency. We can conclude, then, that the effect of the teacher-pupil relation has not been shown to affect the amount of ESP; rather, it is connected with the way psi registers or manifests itself, whether positively or negatively.

In this experiment, by having the teacher introduce the test as part of the classroom activities, it was hoped that, rather than having to induce rapport, it would already be present in the normal classroom situation. The results appear to justify the hypothesis that this type of testing situation is effective in producing successful ESP results. It may be advantageous in future research to use a situation where rapport is not created simply for the ESP task alone but is also an integral part of the life-relations of the persons taking part in the experiment.

APPENDIX A

LETTER OF INSTRUCTION TO THE TEACHER

To the Teacher: (Please follow as closely as possible).

Please do not tell the class to be given this test anything about the test prior to the time it is given. The envelopes are not to be opened by ANYONE, teacher or student. The scoring will be checked here at the Laboratory. At the beginning of the period the test is to be given, the five cards enclosed could be scotch-taped to the blackboard so that all the students might see.

Administration of Test

How many of you have ever had a true-false type of test? (Allow

a minute for class to talk it over.) Have you ever wondered, when you weren't sure of the answer, whether or not you ought to guess? The same thing often happens in a game—say basketball. If you don't know what your opponent is going to do, should you try to guess his move? (Allow minute for class to talk it over.) Do you know that according to the mathematical laws of probability, you can guess and get a certain proportion right? This has been proven by scientific experimentation. BUT it has ALSO been proven that SOME people have the ability to guess far ABOVE chance, that is, much better than average. Today we're going to find out how well YOU can guess.

Here's the way we're going to find out. (Refer to the five cards, bearing the symbols to be used, which you have placed in front of the class.) These five cards represent symbols: a star, a square, a circle, a cross, and wavy lines. Look at them and get them in your mind. A star, a square, a circle, a cross, and wavy lines. Got them in your minds? Fine! Now, I'm going to pass out these envelopes to you, but please do not write on them until I tell you to do so. (Pass them out.) Does everyone have a pencil? (If or when everyone does, continue.) Now, on the white sheet on the envelope: Where it says subject, write your name; where it says observer, write my name; where it says date, write the date of today; and where it says time, give the present time.

You will observe that the white sheet on which you are writing has 10 sections running crosswise or horizontally, and that these 10 sections are made up of columns marked "CALL" and "CARD." Going down each column vertically, you will notice that there are 25 spaces. (Be sure that they all comprehend this.) On the inside of the manila envelope, which you are NOT to open, there is a sheet exactly like the one in front of you on the outside of the envelope. The only difference, and here is the point of this guessing experiment, is that on the sheet on the inside of the envelope, symbols like those on the board are recorded in the "CARD" column, of the first five columns. NOW, to see how good YOU are at guessing, you are to put down one of the five symbols in the CALL column which you think will match the one on the card column on the inside of the envelope. Do you understand: For example, if you think the symbol in the first space in the first column of the sheet in the envelope is a star, then you should match it by putting the symbol "star" in your call column. You are to begin and work DOWN the column. Will

you please note the sentence above the columns. There you will see how to write your symbols. (Go over this aloud with them.)

OK. Now we're ready to go. The way you want to do this is entirely dependent upon you. Some of you may want to think a moment before you put down your guess; some of you may want to put down the first symbol that comes into your mind. Once you put down a symbol, that is, make a guess, it isn't a good idea to change or erase it. If you guess that two or even three of the same symbol come in a row, put them down that way. Don't go back and count how many of each symbol you've recorded. Put down the first thing that comes into your mind. If you can clear your mind of all thought and relax, the symbols may come to you more readily.

When you finish putting down your 25 calls in column 1, go to column 2, then to 3, 4, and 5. DO ONLY THE FIRST FIVE COLUMNS. When you are all done, turn your envelope over and just sit quietly please until the rest finish. Once we start, there should be no talking as this would be confusing to you all. (Please say this with conviction.) I hope you'll do your best to prove to *me* what a good guesser you are. It's important to *me,* and I know you can do well if you want. *I'd* really like to know how good you are. OK, let's start! (No talking now until done.)

(After everyone has finished.) On the back of your large envelope, you will find a smaller white envelope. In it you will find a questionnaire which you are asked to HONESTLY answer. After you have done this, place it back in the envelope and seal the envelope. This questionnaire, along with your manila folder, will be sent out of town right away to be checked. The questionnaire is a *private* matter between you and the experimenter, so please do answer honestly. When you've finished, please pass your material forward.

(Collect their material, add yours, and please return to):

M. Anderson
Parapsychology Laboratory
College Station
Duke University
Durham, N. C.

APPENDIX B

(The numerical rating for each item is inserted in the box beside the appropriate answer. The boxes were left blank, of course, when the student received the questionnaire.)

STUDENT QUESTIONNAIRE

1. Assume that a new course, one that you would like to take and could take, is being offered next semester. Two equally capable teachers, both of whom you know, are being considered for teaching the class. Whom would you prefer to have teach it,

   [1] A. The teacher of this class, or

   [0] B. The other teacher you know?

2. Did you start out the year in this class

   [1] A. Expecting to gain something from the teacher, or

   [0] B. Expecting nothing, as this is just one more class and one more teacher necessary to graduate?

3. Do you start out in most classes with View A [ ], or View B [ ], as stated above?[4]

4. Do you feel that this teacher

	YES	NO
A. Understands	[1]	[0]
B. Likes	[1]	[0]
C. Believes in	[1]	[0]

his students?

5. Would it be easy ordinarily to go to sleep in this class?

YES	NO
[0]	[1]

6. Does this teacher

   [2] A. Stimulate you, make you want to find out about a lot of things,

   [1] B. Teach you only what's in the book, that is, only the "facts,"

   [3] C. Combine the above two, or

   [0] D. None of these?

7. If you had a personal problem you had to discuss with a teacher, would you want to talk it over with this teacher?

YES	NO
[1]	[0]

[4] The authors realized, after the questionnaire was sent out, that Question 3 did not apply to any specific teacher or class. They decided, therefore, that when the data were received, this question would be omitted from the evaluation.

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THE RELATION OF PK OBJECT AND THROWING SURFACE IN PLACEMENT TESTS: I. PRELIMINARY SERIES

By L. C. WILBUR AND G. L. MANGAN[1]

ABSTRACT: This research was conducted to find out whether the roughness of the surface over which an object moves in a PK test affects the subject's scoring; that is, whether the extent and frequency of interruption of the rolling objects is related to the degree of success. For each trial, a subject released 10 balls at a time from an electrically controlled container. The balls rolled down an incline runway, which was surfaced with small steel shot, and scattered into 12 slots at the bottom of the runway. The subject tried to "will" them to go into the six slots on the left, or the six slots on the right, depending on which side was the target. Altogether, in the two series reported, four subjects released 1000 balls each, half for the left side of the runway as target, and half for the right.

It was originally intended to compare the results with runways having surfaces of very different degrees of roughness. For practical reasons, however, the same runway was used throughout, but the size of the balls was changed. In Series 1, they were 5-8-inch glass marbles; in Series 2, they were 3-8-inch steel balls. The surface gave a much greater degree of interruption to the small steel balls than to the marbles and provided relatively the same type of physical comparison as had been originally planned.

The overall results were not above the chance level. There was a significant decline effect, however, from the first to the second half in Series 1. While the greater interruption in Series 2 failed to show any added advantage, no conclusion can be drawn.

Prof. Wilbur teaches in the Department of Mechanical Engineering at Duke University. Dr. Mangan, although a member of the Parapsychology Laboratory at the time of the research, is now in the Department of Psychology of Queen's University, Kingston, Ontario.—Ed.

INTRODUCTION

IN MOST of the reported placement-PK tests, spherical or cubic objects were released by an electrical mechanism to fall by gravity down a slope onto a flat surface, which was marked off into equal target and non-target areas. The purpose of such experiments is to investigate the subject's ability to direct the movement of the objects so that they come to rest in the designated target area of the flat surface.

The first placement-PK test reported was that of Cox (1). In this experiment the apparatus was simple and the objects used were thrown from a cup. The throwing area was divided into 252 squares,

[1] Dr. Mangan's part in this investigation was carried out on a grant from the Parapsychology Foundation of New York.

each of which was assigned a number from 1 to 6. A target number was selected for each release and the subject attempted to influence the objects to fall onto the target squares having that number. The test materials were quarter-inch clay cubes, half-inch celotex cubes, air rifle shot, and five-eighths-inch commercial dice. The dice and cubes gave slightly suggestive positive results, the spherical objects, a small negative deviation.

L. E. Rhine (**11**) used a similar procedure in a placement-PK experiment, except that in her case the throwing area was divided into only two sections. The apparatus was constructed in such a way that the objects were released down an incline and came to rest with approximately equal frequency on the left and right halves of the surface. The test materials were marbles, dice, and coins. The overall results were nonsignificant. Greater success, however, was achieved with coins and dice than with marbles.

Cormack (**9**) and Forwald (**3, 4, 5, 6, 7, 10**) have reported experiments using the two-area placement technique which gave significant results. The apparatus used by both experimenters was similar to that of L. E. Rhine in most essential respects, and in both cases cubes were used. In the Cormack experiment the cubes were of lead, plastic material, and wood; in the first Forwald experiment they were of wood, and in later series of plastic, aluminum, paper, and steel.

A consistent finding from the large amount of PK research, whether placement or throwing for specified die faces, is that PK has been observed to operate only on objects moving over a surface in interrupted motion. It should follow logically from this that the geometry of the test materials, or of the apparatus as it affects the dynamics of the moving objects, would have some bearing on the results obtained. Greater success with rough than with smooth surfaces, and with cubic than with spherical objects, therefore, might be anticipated; the latter, in fact, has proved to be a fairly consistent experimental finding. Cox (**1, 2**) and L. E. Rhine (**11**) both found some differences in results when cubic and spherical objects were used, which suggested that PK could more easily act upon the cubes. Since there seems to be no appreciable difference in results with dice of different sizes and weights (**8**), nor with cubes of different materials (**3**), it is unlikely that the cubic-spherical difference is due in any way to size, weight, or other material differences

among the objects used. It is possible that frequent interruption in motion produces transitory periods when the object is in a state of unstable equilibrium. If so, during the instant when such a condition exists, the forces required to influence the further motion of the object and thus to determine the final "decision" would be extremely small and essentially independent of weight.

Since the shape of the object used in PK experiments seems to have some bearing on the success achieved, there is the question of whether such a difference is due to purely physical factors, to psychological factors, or to a combination of both; that is, whether the dice, because of their physical construction, allow PK to operate more efficiently as they bounce down a slope than do balls moving down the slope in relatively uninterrupted motion, or whether the difference is due to subject preference for dice over spheres in this type of experiment. If the former case applies, a pressing research problem is the extent to which the number and magnitude of these interruptions in the motion of the PK object are related to PK success. The experiment reported in this paper is a tentative first step in approaching this problem.

Procedure

The experimental objective was to compare releases of balls in two series, in the second of which there would be a much rougher surface on the runway. This was planned so as to provide a greater degree of interruption to the progress of the balls in Series 2.

The apparatus was similar to that used in previous placement experiments. The device was framed from two-by-fours and was placed on a sturdy three-by-six-foot laboratory table with two shelves. The container from which the balls were released was a trough constructed of a piece of angle iron sloped at approximately 5°, placed at the top and in the middle of a sloping board. The sloping surface measured approximately 18 inches long by 15 inches wide, and was angled at approximately 15°. The incline surface consisted of a glass plate mounted on heavy plywood and coated with BB shot glued on with glyptol. At the lower end of the board, there was a box divided into 12 slots, which were open toward the sloping surface and which equally subdivided the board. Slots 1 to 6 were designated "left side," slots 7 to 12 as "right side." The side of the box opposite the release mechanism was removable, and

when this was raised after the count was made, the balls dropped into a removable collection box.

Before each release 10 balls were placed in a single row in the angle iron trough. They were held back by an electrically operated door. To release them, the subject pressed an electric contact button at the end of a five-foot cord which was anchored at the other end to the underside of the table. The electrical release wire was always kept slack to avoid exerting any possible bias on the apparatus. Upon being released, the 10 balls rolled down the incline and scattered into the slots. Right and left targets were alternated throughout the series.

The experiment was conducted in a laboratory room in the College of Engineering at Duke University. In spite of care in leveling the equipment, there was a consistent bias toward the left side. This was probably caused by a very slight fixed angle of the launching chute relative to the inclined surface. Since the design of the experiment called for the alternation of right and left sides as targets an equal number of times, such a bias was offset and merely resulted in over-estimation of the variance and, consequently, under-estimation of the CR's.

All contact with the apparatus was avoided during the session except by subjects pressing the release button and readjusting the release mechanism, and by the checker in releasing the balls from the slotted box. After the subject had released the balls, one person, assigned as checker, called out the number of balls in each slot to a recorder who filled in the record sheets. The subject, who had an unimpeded view of the slots, kept track of the recording while the checker was calling out. The recorder noted down the number of balls falling in each of the six slots on the target side, added them up, and recorded the total deviation from the expected five. When this was agreed upon by the subject and the checker, the balls were released from the slots and handed back to the subject who returned them to the release chute. In one session, one subject preferred to remain ignorant of his score until the set was completed. In this instance an additional checker was employed.

The unit of testing was a "round," which was one release of 10 balls for a specified target. A "set" was 10 "rounds," five for each target side. Two "sets" constituted a "session." Five "sessions"

for each subject constituted a "series." Each subject thus released a thousand single balls, 500 for each target side, in each series.

The same procedure was followed throughout except for a difference in the surface texture of the runway. It was decided that rather than replace the original surface used in Series 1, the same result might be achieved by using smaller balls on the same surface, and this was the change actually introduced. The five-eighths inch glass marbles used in Series 1 were replaced in Series 2 by steel balls measuring three-eighths of an inch in diameter. The relative roughness of the surface in Series 2, therefore, was much greater. As there may well be an optimum relative roughness for PK, the second condition might have been too fine or too coarse. Many more data will be needed to determine this.

Four subjects took part in the experiment, which was completed during the period Dec., 1955, to Feb., 1956.

Results

Four independent evaluations were planned in this analysis. All the P values, therefore, have been allowed a four-fold correction.

The two primary evaluations were based on the overall deviations in both series and the difference between them. The 4,000 single throws in Series 1 (1,000 for each subject) gave a total deviation of +31; the deviation in Series 2 was −6, neither deviation being significant. The difference between the deviations in the two series was not significant, and neither were the deviations of individual subjects. Results for the individual subjects and for the total experiment are shown in Table 1.

A comparison of the deviations in the first and second halves of each series was made. In Series 1 the total deviation in the first half (that is, the first five sets) was +64, which has a CR of 2.86 (P=.016). The deviation in the second half was −33. The difference between the first and second halves of Series 1 was 97 (CR=3.07; P=.008). In Series 2, the difference between the first and second halves was not significant.

A further evaluation, related to the series decline, was the frequency of occurrence of positive and negative set scores in the first and second halves of each series. This analysis was not planned at the outset, but it was tried at the end of Series 1 as another measure of series decline and it was then used for the same purpose in the data of the entire experiment. In the first half of Series 1 (which

Table 1

GENERAL RESULTS IN TERMS OF INDIVIDUAL SET SCORES AND TOTALS FOR EACH SERIES

Series 1						Series 2					
	Subjects						Subjects				
Set	J. R.	G. M.	L. W.	R. L.	Total	Set	J. R.	G. M.	L. W.	R. L.	Total
1	3	5	0	5	13	1	4	0	1	4	9
2	3	11	4	−2	16	2	−4	−3	−5	2	−10
3	−1	7	−2	6	10	3	0	−6	2	−5	−9
4	5	3	9	0	17	4	1	3	−8	0	−4
5	−2	11	−3	2	8	5	3	1	2	2	8
Total	8	37	8	11	64		4	−5	−8	3	−6
6	2	1	2	0	5	6	−3	−4	4	0	−3
7	−4	1	−1	2	−2	7	−2	−6	−3	1	−10
8	6	−3	−2	−1	0	8	4	−2	9	0	11
9	−6	−4	−7	−3	−20	9	−8	−4	−2	4	−10
10	−5	−8	3	−6	−16	10	−1	0	12	1	12
Total	−7	−13	−5	−8	−33		−10	−16	20	6	0
Pooled Total	1	24	3	3	31		−6	−21	12	9	−6

gave a positive overall deviation) there were 13 positive set scores, 5 negative. In the second half there were 7 positive set scores, 12 negative. In Series 2, although the overall deviation was slightly negative the same general trend was apparent. In the first half of

Table 2

FREQUENCY OF POSITIVE AND NEGATIVE SET SCORES IN THE FIRST AND SECOND HALVES OF SERIES 1 AND 2 POOLED

	Pos.	Neg.	
First Half	24	11	34
Second Half	14	22	36
	38	32	70

$\chi^2 = 6.29$ (1 d.f.); $P = .05$.

Series 2 there were 11 positive scores, 6 negative; in the second half, 7 positive, 10 negative. The pooled results are shown in Table 2. The associated chi square is 6.29, which has a probability of .05.

A final evaluation, related to the rate of success in each series, was a comparison of the scatter of the marbles when right and left sides were targets. The difference between the mean values for both sides in each series is nonsignificant. The spread of the marbles into the slots was very little greater than would be expected by chance. No comparison of the spread of balls between Series 1 and 2 is valid, since there was a greater spread in Series 2, due solely to the physical difference between the two sizes of spheres used.

Discussion

The high positive scoring in the first half of Series 1 and the significant first- and second-half difference in Series 1 indicate a considerable chronological decline (see Fig. 1). This, together with the consistency analysis, which held up for both series, was the main effect. Such a decline effect has been a marked feature of PK test results and is, therefore, in line with expectation.

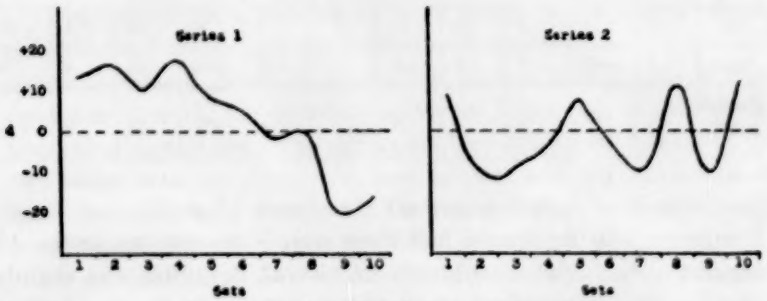

Fig. 1. Scoring trends by sets within each series.

Comparison of the results of the two series gave only an insignificant difference. Even if this difference had been significant, however, it is doubtful whether it could have been interpreted as indicating any sort of real relationship between the degree of interruption of the marbles and PK success. The experimental design was faulty in that a complete series using one surface was followed by a complete series using the other, rather than alternating the surfaces within the sessions. Any negative result in Series 2 might have been due to a simple decline effect, and any genuine advantage

to PK from the relatively rougher surface might have been obscured by such a decline. The method was used, however, since it had certain procedural advantages in that it did not require constant changes of surfaces during the experimental sessions, which would have proved extremely difficult. The introduction of smaller marbles in Series 2 rather than the use of a rougher surface, although it met the required physical conditions, might have introduced extraneous psychological factors.

These shortcomings, however, have been taken into account in further experiments now under way. All that can be said for the experiment reported here is that some PK effects were demonstrated in a placement test using spherical objects, and that the test served to emphasize some procedural difficulties and point up defects in the experimental design. Obvious improvements have been incorporated in a new experimental design.

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QUANTUM PHYSICS AND PARAPSYCHOLOGY[1]

By C. T. K. CHARI[2]

ABSTRACT: Dr. Chari begins his discussion with the suggestion made by certain leading physicists that parapsychology might profit by considering the suggestions and analogies provided by quantum mechanics. He ends with the judgment that insofar as the philosophical implications of quantum mechanics are clear, they do not afford much interpretative aid to parapsychology. In his review he considers the attempt made to apply the indeterminacy principle beyond the boundaries of physics proper, and he discusses the significance for precognition of the breakdown in theoretical physics of such classical concepts as causality and the irreversibility of time. He comes to the verdict that these hypotheses and postulates of modern physics make no specific contribution to the solution of the problems of parapsychology. Finally, Dr. Chari critically examines certain recent attempts to deal with the relation of psi phenomena and physics and recognizes that, for the present, the best reaction is a confession of ignorance. Quantum mechanics does, however, make a positive contribution to parapsychology, he adds, in the lesson of restraint it offers to those who would hastily reject psi phenomena on physical grounds.

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Two distinguished physicists, Pascual Jordan (**17,** pp. 3-7) and Henry Margenau (**26,** pp. 14-15) have recently pointed out that quantum physics can offer the parapsychologist some worthwhile suggestions and analogies even if it cannot solve all his problems. To go no further, Bohr's "complementarity principle" seems to imply a "loosening of the laws of causality" (**17**), a replacement of "determinism" by "indeterminism," which may have important consequences for the "steering" or "regulation" phenomena of living organisms. If psi phenomena are "triggered" by events of atomic dimensions, they may be amenable to a statistical rather than a causal treatment. In the second place, recent developments in quantum

[1] Dr. Chari has furnished a number of mathematical footnotes to the paper which have been omitted. The points at which they occurred are indicated by asterisks. Readers who wish to have these footnotes for the further clarification of the article may obtain them by writing to the editor.—Ed.

[2] I am grateful to the Parapsychology Foundation of New York City for the award of a research stipend, and to Dr. Gardner Murphy, its General Consultant, for much encouragement in my attempts to grapple with some theoretical issues in parapsychology. I have to thank Dr. Adolf Grünbaum, Associate Professor of Philosophy, Lehigh University, Bethlehem, Pa., for the mimeographed copy of his valuable paper "Carnap's Views on the Foundations of Geometry" which he sent me. To the Syndicate of Madras University I am indebted for its permission to use some material in my doctoral dissertation entitled "On Some Spatial Representations of Time."

dynamics suggest that, in dealing with single particles, there may be "reversals" of the temporal "before-after" relationship. Margenau (**26**) has mentioned the Feynman-Stückelberg hypothesis which postulates positrons as particles "moving backwards" in time. Jordan has singled out observations suggesting that the "explosion" of an atomic nucleus may occur *before* it is hit by a fast-moving particle. All this may have a bearing on the problem of precognition. Thirdly, according to both the distinguished physicists, quantum physics can bring home to the parapsychologist not only the general need for training in modern applied mathematics, with its "abstract representations of reality" (**26**), but the urgency of exploring specific hypotheses. Margenau has referred to the symbolic interpretation of Schrödinger's wave equation as a "probability wave." If the Ψ of wave mechanics represents "information," then the form of the equation suggests that "information" can spread instantaneously throughout space and become "concretized" as "fact" (i.e., certainty) when we conduct measurements. Margenau has made it clear that he does not favor the hypothesis himself. "However," he says, "if this theory with all its debatable features could be shown to be useful in psychical research, then its status in physics would be greatly improved."

If quantum physics can furnish the parapsychologist with really useful analogies, he can hope that light will be shed on the perplexities of ESP and PK by pursuing the analogies. If, however, quantum physics is in too transitional and controversial a stage to establish consensus on philosophical issues, and if, moreover, there are basic questions to be asked about all attempted physico-mathematical descriptions of reality, then the hoped-for "synthesis" of physics and parapsychology will not come about by either of them making too many concessions to the other. It is the latter point of view that I should like to argue here. I may as well say at the outset that I am not denying the very great importance of the quantum-mechanical concept to which Jordan and Margenau have drawn attention. My contention is that the *philosophical* implications of quantum mechanics are not so clear that we can grant that modern physics has already gone a long way in the direction of parapsychology. There are *analogies;* but one cannot be too sure of their *interpretation.*

I

We must first of all ask whether quantum physicists are agreed that Heisenberg's "indeterminacy" plays a decisive role in the "triggering" of biological events. As early as 1932, Jordan proposed the view that an unpredictable quantum event may initiate a change which can then be magnified to the level of macroscopic visibility. To account for radioactive decay and cellular changes alike, Jordan supposed that one major quantum event, independently of others, can act as a "steering center" in molecular dimensions. The claim is embodied in Jordan's *Treffertheorie.* I. D. London (**21,** pp. 123-49) regards Jordan's *Verstärkung* as a special case of what Langmuir called "divergence"; i.e., the hypothesis that from a small beginning increasingly large effects can be produced. London has proposed various applications of the principle to neurophysiological changes and even psychological developments such as the "Aha!" phenomenon or "insight" and "religious conversion." Unanimous support for a theory conceived along these ambitious lines is lacking. Schrödinger (**34,** p. 87) thinks that, apart from enhancing purely accidental characters in such events as meiosis, natural and X-ray induced mutations, quantum "indeterminacy" plays no great role in biological processes which, if not strictly determined, are at any rate statistically determined. Jordan's speculation remains an interesting possibility. It is difficult to see what precise significance it can have for ESP and PK.

Turning to the alleged "breakdown of the causal principle" in quantum physics, we must ask whether it is a renunciation, restriction, or reformulation of the principle. In two recent papers (**6,** pp. 177-85; **7,** pp. 60-67), I have shown that all the three interpretations have to be reckoned with. Bohr's "complementarity principle" asserts that the ideas of position and velocity in conjunction, which we use in macroscopic descriptions of moving bodies, are not very well suited to atomic phenomena. For systems of atomic dimensions, location and speed are "complementary" but none the less *mutually exclusive* features of the framework of description. If we wish to secure exact localization of particles in space-time, we must give up causality; and if we wish to retain causality, we must surrender space-time descriptions.

Hans Reichenbach (**28,** Part III) has proposed a restriction of language to deal with the "breakdown" of causal descriptions in

quantum physics. Distinguishing the actual results of observation (e.g., the appearance of a photon or light particle on a screen after passing through one of two slits) from "interphenomena" (the theoretical constructions which attempt to tell us what has happened to the photon in its passage through one slit rather than another), he maintains that the "interphenomena" of quantum physics cannot be constructed without causal anomalies. A way out of the difficulty, according to Reichenbach, is to assimilate quantum mechanics to a three-valued logic in which certain propositions about the "interphenomena" are neither "true" nor "false" but carry a third value, "indeterminate." They fall outside a domain of "restricted assertability."

There are physicists like Margenau (**23,** pp. 418-22) who are unable to acquiesce in Bohr's dilemma, "space-time location *or* causality," which condemns physics to a partial agnosticism, and Reichenbach's trichotomous logic which may be convenient but is not obligatory. According to Margenau, physics has already made its choice. It has renounced visualization of particles sharply localized in space-time and accepted an abstractly defined causal continuity. If causal connections are defined on the plane of immediate observation, they must be surrendered in quantum mechanics. If, however, the causally evolving "states" of quantum mechanics are linked to aggregates of observations rather than single observations, a reformulation of the concept of "states" in terms of probability distributions is necessary. In a recent Joseph Henry Lecture (see **19,** p. 90) before the Philosophical Society of Washington, Margenau has presented his view as a sort of synthesis of the "deterministic" (or "mechanistic") and the "indeterministic" (or "formalistic") views.

It is necessary to point out to non-specialists that the principal forms of the Schrödinger equation used in wave mechanics satisfy the requirement of dynamic reversibility; they do not distinguish the "forward" from the "backward" direction of time, i.e. $+t$ from $-t$.* The statement requires qualification only when the quantum mechanical system is subjected to "observation"; i.e., when it is coupled indivisibly to a classically describable system undergoing irreversible changes in the process of securing "information." As Von Neumann, Watanabe, and Grünbaum (**15,** pp. 564-66) have shown, the Schrödinger equation for this special case behaves

asymmetrically with respect to "past" and "future." Watanabe has interpreted the situation as signifying that the time of physics by itself has no "direction"; it is the "observer" who confers "directionality" on it. Schrödinger, however, has demurred to the use of the metrogenic quantum asymmetry as a basis for explaining phenomenal (macroscopic) irreversibility. He has remarked: "Surely the system continues to exist and to behave, to undergo irreversible changes and to increase its entropy in the interval between two observations. The observations we might have made in between cannot be essential in determining its course." (See **15,** pp. 565-66.)

The processes of classical dynamics are essentially reversible (**18**, p. 292). Only thermodynamic irreversibility (the law of entropy distinguishes $+dt$ from $-dt$) enables us to fix what Eddington called the direction of "time's arrow." It may be argued that the "irreversibility" reflects the non-availability of the relevant data pertaining to the fate of the individual particles. Has the situation been basically altered by quantum mechanics? Max Born (**2**, pp. 71-73, 109-14) has attempted to deduce a genuinely monotonic increase of entropy with time from the fundamental principles of quantum mechanics. The gist of his argument may be expressed by saying that quantum physics, having accepted "partial ignorance on a lower level," need not "doctor the final laws." A. Landé, again, has attempted to derive macroscopic thermodynamic continuity from the quantum transitions for particles which conform to odds regulated by probabilities. (See **19,** p. 91.) It is unlikely that the claim will prove decisive. Watanabe contends that quantum mechanics, like Newton's mechanics, can furnish an irreversible thermodynamics only by admitting a statistical principle of interpretation as an ancillary to its principal dynamic laws.

II

There exist crucial differences of opinion among experts, not only about the alleged "breakdown" of space-time and causal descriptions in quantum mechanics, but about the interpretation of particular symbols like Schrödinger's Ψ. What is the plausibility of the theory (to which Margenau has referred) which interprets the Schrödinger equation as describing the "spread of information" and its "concretization" into "fact" when we conduct measurements? The situation may be roughly summed up thus. A too narrow ad-

herence to the earlier theory of "orbits within the atom" would lead one to expect that the statistics of quantum mechanics will yield probabilities which are different from zero only for points that define the "orbits." Surprisingly enough, the values of the Schrödinger function Ψi differ from zero *everywhere* in space except for certain "nodal surfaces." For any "stationary state," it is probable that the electron (so far as the determination of its position is concerned) can be found at any distance from the nucleus. The "orbits" reappear in wave mechanics only as the maxima of the functions $\Psi i(xyz)^*$ contributing to surfaces that have a certain resemblance to Bohr's circles and ellipses. Metaphorically speaking, the "orbits" exist only as a diffuse "statistical cloud"; measurements are seemingly attempts to "condense" the "cloud." Or, in the language of "wave packets," to which Margenau has also referred (**26**), the uncertainty of the initial state gives rise to an increasing uncertainty in future states. The "packets" begin to spread outward; with sufficiently large values of t their "dissolution" sets in; any identification of them with "particles" proves impracticable.

A. March (**22,** pp. 165-66, 179-80, 189, 203) has argued, with considerable justification it seems to me, that however plausible the symbolic interpretation of the *non-relativistic* Schrödinger equation as a "probability wave" may appear, it loses much of its plausibility when we turn to the *relativistic* forms. On any relativistic interpretation, the quantum mechanical field (Schrödinger's *relativistic* Ψ) possesses charge, energy, and momentum which are, in principle at least, correlated with the "observables." The relativistic field, so long as it is not quantized, is not associated with the idea of particles, which is an essential presupposition of the non-relativistic Schrödinger wave equation. Only when the field is quantized, particles enter the picture; the process endows the field with properties which are expressible in terms of whole numbers. Nevertheless, there seem to be features of the relativistic quantum mechanical field which correspond to characteristic features of the particle picture. March (**22**) has dwelt on the fact that, in the relativistic treatment, the density can be positive as well as negative, which seems to imply that positive as well as negative particles will be encountered in the particle picture and that the charge rather than the number of particles per unit volume is what we phenomenally apprehend. In all its forms, relativistic quantum mechanics insists on processes involving the creation and the annihilation of pairs. Dirac's original

relativistic wave mechanics appeared to be an exception only by permitting particles of negative energy. The interpretation of the negative energy states, however, within the framework of a "hole theory" entails the assumption of processes which, so far as phenomenal observations are concerned, are presented as if they were identical with the creation and the annihilation of pairs.

It is far from evident that a relativistically invariant wave mechanics will be especially relevant to parapsychology. Even the non-relativistic wave mechanics does not immediately suggest either ESP or PK. ESP is not, so far as we have been able to study it, a *diffuse* spread of "knowledge" throughout space. The psychological variables (personality attributes) contribute essentially to its spontaneous and experimental manifestations. How are these psychological variables to be fitted into the "probability wave" picture? Quantum mechanics has been constructed to suit the special needs of physics. The possible isomorphism between the laws operating at two levels, one "higher" and the other "lower," may blind us to intrinsic differences between them.

III

Let us now consider briefly the "time reversals" postulated in the Feynman-Stückelberg theory. (See **24**.) We have noticed that relativistic wave mechanics, in all its forms, postulates creation and annihilation of pairs. Pair production is closely related to the Bremsstrahlung process (**16**, pp. 133-34); in the former, a photon interacts with an electron in a negative energy state in the neighborhood of a nucleus; in the latter, an electron in a positive energy state interacts with a photon in the neighborhood of a nucleus. The two processes differ in the sign of the total energy of the initial and the final states; in the Bremsstrahlung, both initial and final states of the electron are positive energy states. Dirac's concept of negative energy states is implicit in the derivation of the well-known Klein-Nishina formula (**16**, pp. 151-52) which gives the "cross-section" of the Compton process as a partial absorption coefficient.

The Feynman-Stückelberg theory provides an alternative mathematical description of pair production. It postulates particles moving along an abstractly conceived four-dimensional space with three space coordinates and one time coordinate. An electron moving along the four-dimensional continuum in such a way that its time coordinate increases is to be interpreted as a negatron; if its time

coordinate decreases, it is to be viewed as a positron. Let us consider the points P_1, P_a, P_b, P_2 on a "world-line" in the four-dimensional continuum ordered by the *temporal* relations: $P_1<P_a$, $P_1<P_b$, $P_b<P_2$, $P_1<P_2$, but $P_a>P_b$. ($>$ signifies "after" and $<$ signifies "before.") Pair production may be pictured in the following terms (**16,** p. 156): (i) A negatron moves from P_1 *to* P_a. (ii) A pair is produced at P_b. (iii) The positron member of the pair moves from P_b to P_a. That is, its time coordinate *decreases* ($P_a>P_b$); it annihilates itself and the negatron that has arrived at P_a from P_1. (iv) The negatron member produced at P_b proceeds to P_2. To single out the philosophical issue (**15,** p. 554, footnote) which *may* be relevant to parapsychology, the mathematical hypothesis suggests that a particle can travel both forwards and backwards in macro-time, although its proper micro-time is linear. It can be in two different places at instants which are macroscopically "contemporaneous" having travelled both forward and backward *between* the "contemporaneous" instants. The situation depicted violates the criterion for the "one-wayness" of time formulated by Margenau (**23,** p. 160). Suppose a particle occupies the successive positions of space x_1, x_2, x_3, at the successive instants of time, t_1, t_2, t_3. "Irreversibility of motion" is the demand:

$$x_2 \neq x_1 \text{ and } x_3 \neq x_2,\ x_1 < x_2 < x_3.$$

The "one-wayness" of time, on the other hand, is the demand:

$$t_2 \neq t_1 \text{ and } t_3 \neq t_2,\ t_1 < t_2 < t_3.$$

There is nothing to show why the first demand must, in principle, be fulfilled; why not $x_3=x_1$, $x_2 \neq x_1$? The non-fulfilment of the second demand, i.e., $t_3=t_1$, $t_2 \neq t_1$, would mean that, at one and the same instant, a particle can be in *two* places; or, *between* two "contemporaneous" instants of time, there can be other instants (**15,** p. 554, footnote), which contradicts our usual notion of a time ordered by "linear betweenness."

Reichenbach (**29,** pp. 150-53) has offered three philosophical comments on the Feynman-Stückelberg hypothesis. In the first place, he says that it should remind us of the dynamic reversibility of the quantum mechanical equations. The symbol t has no unique "direction" in the equations; it acquires its usual macroscopic significance only after statistical considerations have been brought in. Secondly, too much should not be made of the Feynman-Stückelberg "time reversal" so long as it remains one of *alternative* mathematical

descriptions of the observed correlations. In all attempts to construct the "interphenomena" of quantum mechanics, Reichenbach observes, causal anomalies make their appearance. The Feynman-Stückelberg "time reversal" and Dirac's "negative energy states" enforce the same lesson in two different ways. Finally, the life of the positron is brief, infinitesimal; we cannot generally suppose that particles moving in the "reverse direction" of time contribute decisively to phenomenal (macroscopic) time. Beyond suggesting that *if* our conventional ordering of time by "linear between" breaks down when we confront certain quantum dynamical problems, it *may* fail elsewhere too, the Feynman-Stückelberg hypothesis makes no specific contribution towards a solution of the parapsychological problem of precognition.

IV

To realize the true magnitude of the problem of precognitive psi, we should bestow a glance on the divergent views about time proposed recently by two philosophers of science, Grünbaum (**14, 15**) and Reichenbach (**29, 30**). Both set out from distinctions which are highly pertinent to the problem. An order may be asymmetrical and transitive without intrinsically distinguishing "directions." The geometrical line exhibits the same intrinsic structure in each of two opposite directions. Nothing in the line indicates that a given point is to the "left" rather than to the "right." The "direction" of a line in geometry has to be fixed conventionally (**5**). Nonetheless, the asymmetrical ordering of the points on a line is an intrinsic property resulting uniquely from "linear between." One and only one of three given points can be linearly *between* the other two (**5**). An asymmetrical order of this type should be distinguished from one which is not only asymmetrical but intrinsically distinguishes *each* of *two* directions from the other. The time of ordinary experience is asymmetrical and transitive; and it intrinsically distinguishes the direction "before" from the direction "after." Besides, it seems to exhibit a "march of events" or "becoming" in a *privileged* direction, "future." Our ideas of "becoming" are perhaps obtained by a natural extrapolation of our experiences of time-successions bounded on one side by a past which can be only "remembered" and, on the other side, by a future which, under favorable circumstances, may be "anticipated" but can never be experienced or remembered.

Does modern physics (including here relativity as well as quantum physics) admit of a unique past-present-future series? Grünbaum (**14, 15**) and Reichenbach (**29**) have given different answers to the philosophical question. Grünbaum finds no adequate grounds, either in relativity or in quantum mechanics, for belief in a *unique* ordering of the time-series by past-present-future. He maintains that this feature of our ordinary experience can be only a macroscopic phenomenon tied up with the generally operative thermodynamic irreversibility. According to him, the law of entropy not only fixes "time's arrow" but determines the psychological distinction between "memory" and "anticipation." "Information-gathering" systems participate in the processes of nature. We accumulate experiences and memories in the way prescribed by the law of entropy. Grünbaum (**15,** p. 559) illustrates his contention by considering an interesting problem posed by Moritz Schlick. I propose to state briefly the problem and the solutions sketched by Schlick and Grünbaum, since they have a bearing on the problem of precognition. Schlick asked why, if we see footprints on a beach, we can infer that a person *was* necessarily there in the past but not that somebody *will be* necessarily there in the future. Schlick offered the explanation that the structure of the past is inferred from the spatial arrangement of objects and not from the extent to which the energy has been dissipated. He granted, however, that broadly speaking, spatial traces are produced in accordance with the entropic principle. In the example under discussion, the kinetic energy of the person's feet became dissipated in the process of arranging the grains of sand in the form of relatively persistent footprints. Grünbaum amends the explanation by saying that it is when we encounter systems which, though isolated now, and behaving as if they *might* well remain isolated, nevertheless suggest that they *could not* have been always closed, that we infer a portion of the past from the present. A *partial effect* produced in a system, while it is open, permits an inference, on entropic grounds, concerning an earlier interacting event which was the *total cause*. In the illustration considered, the stroller on the lonely beach is *inferred* from the patent fact that the degree of order possessed by the sand, with the footprints on it, is much higher than it ought to be if the beach had always been a closed system. Grünbaum concludes that, consistently with the entropic hypothesis, we can have a record in the form of "memory traces" only of events

which are *past* in relation to a particular "here-now." A record or information can be treated as negative entropy or "negentropy" acquired at the expense of the environment of which the record is a part. "Information-gathering" systems are "open" systems and are a part of branch systems characterized by positive entropy increase. A necessary entropic condition for *knowing* the future is the specification of the *total cause*, i.e., the state of a *closed* system. While the total cause can be inferred from a partial effect, the total effect cannot be inferred from a partial cause.

Reichenbach agrees with Grünbaum that relativity and quantum mechanics, by themselves, do not provide unambiguously for a unique past-present-future series; but he holds that they are *not incompatible* with the assumption, especially if we adopt a probability interpretation of macroscopic causality according to which the past is "objectively determined" while the future is "objectively undetermined" in virtue of the non-existence of the requisite complete set of partial causes a knowledge of which alone would render our predictions absolutely certain. It may be said that relativity rejects absolute simultaneity and, therefore, casts doubt on a unique "now." Reichenbach meets the difficulty by regarding the present as the class of events which are not causally connectable with a particular "here-now." He shows (**29**) that the quantum indeterminacies can be utilized in support of the hypothesis. *All* the events of the past can, in principle at least, be recorded; the indeterminateness of the future, on the other hand, is bound up with its inherent unpredictability. The "present," which intervenes between the past and the future, is the "threshold moment" at which the indeterminate becomes determinate. "Becoming," in fact, is *becoming determinate.* Reichenbach's valuable analysis lends support to C. D. Broad's earlier arguments (**3,** Part I, Ch. II; **4,** Vol. II, Chs. 35 and 51) constructed to show that a series characterized by past-present-future (McTaggart's "A-characteristics") cannot be *unambiguously* translated into a series characterized only by an asymmetrical before-after (McTaggart's "B-characteristics"). One remembers that Eddington, in correspondence with J. W. Dunne and on other occasions, held that, although the Minkowski space-time does not seem to provide for any absolute "becoming," the feature is probably there and has an integral connection with our "free will" and "choice" and that E. A. Milne likened time not to the flight of *one* arrow (the

crude simplification cannot survive Einsteinian or Kinematic relativity) but to the flight of *several* arrows.

To come to the crux of precognition: Where imaginative anticipations and inferential predictions based on accumulated experiences and memories are not available, then, according to Grünbaum's view, no significant statement about the future should be possible at all. It is true that he allows a possible occasional "reversal of memory" (**15,** p. 568) in view of the fact that the entropic restriction is only a probability restriction; but these occasional lapses, even assuming that they happen, would be a "going backward" in time and not a "going forward." Grünbaum opines that the occasional cases may be encountered only in psychiatric literature; the chances of "survival" of the patients, even under the sheltered conditions of modern hospitalization, would be negligible. It is clear, therefore, that the causal and entropic theory of time has no relevant analogue of precognition to suggest. Any proposed analogy, within the framework of the hypothesis, would be in the shape of the stroller who obligingly left his footprints on the lonely beach *before* he actually came. In the experimentally controlled situation, precognition reveals itself in the "hits" and "misses" centering around perfectly randomized "targets," successful "guesses" at which, according to all scientific expectations, cannot be *significantly more* or *less* than a certain number, with certain limits of error prescribed by the refinements of probability theory.

The problem is even more formidable on Reichenbach's theory. If the future is unpredictable because of its inherent indeterminateness, and if causal connections are resolvable into "successions of probability," then the significant precognitive *departures* from all prescribed "probability expectations" seem to admit of no explanation at all, not even in outline. C. D. Broad, H. H. Price, and C. W. K. Mundle have discussed the ontological issues stemming from the difficulty.

A. G. N. Flew has told us that much talk about "effects preceding their causes" in parapsychology may be a hopeless muddle, especially if the time-honored "principle of causality" is little more than a tautology. He finds that Jung's theory of non-causal "synchronicity" is a "premature counsel of despair" (**12,** p. 200). "Advanced sciences," according to Flew, have long ago replaced "causal laws" by "functional laws." A functional law is confined to the statement

that x varies with y or with any number of other variables according to a prescribed formula. Such laws are not necessarily interpretable in terms of "causes preceding their effects," much less in terms of causes which influence their effects without any intervening spatio-temporal gaps (**11,** pp. 128-29; **12,** pp. 198-201). I should cheerfully agree with Flew about all this. But, even in the most "advanced sciences" (surely mathematical physics must rank as one), what, I ask, is the analogue of the kind of "functional relationship" between a "successful precognitive guess" and its "target" which we need to be able to deal with experimental precognition? Flew sometimes seems to imply that ESP "hits" are just "statistical corrections"; it should be borne in mind that what has to be explained is the *departure* from all "statistical expectations." One remembers Bertrand Russell's "mnemic causation"; but that would hypothetically bridge a gap only between the past and the present. What is the warrant, in either biology or psychology (outside the "parapsychological," of course), for postulating an analogous principle linking the present and the future? I do not see how any overhaul of our language, without a radical alteration of the current scientific framework, will solve the problem.

I own I do not understand how the "images" postulated by C. C. L. Gregory and Anita Kohsen (**13,** p. 176) which are "informationally unrelated to the time provided by physical clocks" can arise at all in the human mind. "Image spaces" which are determined by "relevancy and coherence" rather than by "physical and temporal location" seem to be little more than a restatement of the problem. An almost similar, if more ostentatious, hypothesis was proposed by the philosopher C. A. Richardson (**33,** Ch. III). He postulated "line-elements" of a four-dimensional space-time analogous to that of Minkowski and speculated on the appearance of "images" in the mind corresponding to the known and unknown probability distributions of "future states of occurrences." His assumptions will not bear scrutiny. R. H. Thouless (**36,** p. 209) wrote:

"It seems likely that a more radical re-orientation of thought is necessary before explanation can be reached. If there is a difficulty in exlaining precognition, there is something wrong with our system of expectations." Obviously; the real trouble lies in attempting to change the system. It is easy, in attempts at an explanation of pre-

cognition, to start with an imposing array of terms and discover how little they accomplish.

I maintain that the issues about "time," "precognition" and "intervention" raised by L. E. Rhine (**31, 32**) are very real and must be faced squarely by philosophers and parapsychologists. It is noteworthy that most researchers who have busied themselves with the problem of precognition, like Richet, Osty, Dame Edith Lyttelton, Saltmarsh, Tyrrell and J. B. Rhine, have confessed to being quite out of their depths: a most salutary confession. Researchers like D. J. West (**37,** p. 139), while confessing to a shade of perplexity, hope for light from new experiments. By all means; let us conduct new experiments; but let us not forget, even for a moment, the Gargantuan theoretical difficulties. G. R. Price (**27**), in the recent attack he has launched against Rhine and Soal, has indicated that at least one reason for regarding parapsychology as the twentieth-century counterpart of astrology is that, within the current scientific framework, one cannot even conceive of a plausible theory of precognition. But can the failure of current concepts be made completely intelligible against the background provided by those concepts themselves? The Feynman-Stückelberg theory warns us that we should not take for granted even those notions of "time-ordering" which have been handed down for generations and without which everyday living would be a perfect maze. Quantum physics can restrain the precipitate skeptic even if it cannot convince him of the parapsychological verities.

V

Before I conclude my paper, I should like to refer to trends which emphasize the tentative and exploratory character of much quantum mechanical speculation in the nuclear domain. Leaders like Schrödinger (**35**) and de Broglie (**9,** p. 641; **10,** Ch. 10, Section 6) have recently been expressing a desire to see quantum mechanics phrased in more deterministic terms. De Broglie refers appreciatively to D. Bohm's attempt to sketch a deterministic hypothesis by utilizing "hidden variables." The theory reduces to the current quantum electro-dynamics for regions greater than 10^{-13} cm., but departs significantly from it within the nucleus. Reichenbach (**29,** p. 129) has criticized Bohm's theory. Yet, it is possible that a theory developed along these lines may meet with better success in future. Interesting attempts have been made, too, to endow the

"smallest length" ("hodon" of the dimensions 10^{-13} cm.) and the "smallest time" ("chronon" of the dimensions 10^{-24} sec.) with an altered status. Heisenberg has introduced an S-matrix (see **8,** Ch. 12) with an associated "fundamental length" l_0. He thinks that the Schrödinger energy operator $H\Psi$ acts as a time derivative and so presupposes continuous changes in time.* If time exhibits a quasi-discreteness, $H\Psi$ may be inapplicable in its present form (**23,** pp. 157-58).

It may be instructive to refer also to March's geometry of a *Kleinster Raum,* the metric of which is defined in a fundamentally statistical form and forbids the use of lengths less than 10^{-13} cm. $= \gamma$ (say).* If $P_1, P_2, \ldots, P_n$ be a series of points at distances equal to γ, then $P_1 P_n = O$. Serious mathematical difficulties arise when we try to interpret the geometry as a non-Riemannian construction giving the metric in the integral but not the differential form. In this geometry, conceivably there can be a circle of zero-radius but finite circumference. March (**22,** Ch. 10) has attempted to avoid unnecessary mathematical complications by substituting physical for mathematical points. He claims that, by the introduction of a statistical metric, we can not only cure quantum mechanics of the "disease of the infinities" (e.g., a cavity filled with radiation, by the Fourier superposition of an infinite number of harmonic oscillators, can yield an "infinite zero-point energy") but tackle those nuclear problems of which current theories can make no sense. Jordan has referred to the anomalies arising in connection with mesons. Suppose we consider the behavior of mesons traversing the layers of matter in one of the lighter elements like carbon, being stopped, and absorbed by a proton in the nucleus. Calculations from the current theories yield an absorption probability time for a meson which is much smaller than the probable time for the decay of a meson into an electron and a neutrino. It should, therefore, be impossible to observe negative decay electrons. But with carbon as a stopping material, such negative disintegration electrons almost equal the positive ones. Actual observations show that the absorption probability is smaller, say by a factor 10^{12}, a serious discrepancy indeed. March attempts to show how the introduction of a statistical metric removes the difficulty of the interaction of a meson with one and only one proton which exists for current theories.

There are abundant indications that quantum mechanics may have to allow for new mathematical departures which would remove some at least of the nuclear anomalies. E. T. Bell (**1,** p. 93) has said: "The quantum theory has digested vast quantities of algebra, digesting some, rejecting others with loathing. . . . What nuclear physics may demand of algebra before it has split the last nucleus may split algebra into even smaller and harder fragments and compress algebraists into even narrower specialists." It should always be remembered that parapsychology deals with observers and events on the familiar macroscopic scale. Anomalies here are of a very serious character and may well remain so when nuclear physics, armed with some fantastically clever algebra, has cracked its puzzles. Certainly we must look forward to the day when physics and parapsychology will outgrow their provincialisms, when the "physical variables" can no more be separated from the "psychological variables." We can be tolerably certain that, when the day arrives, physics cannot survive in its present form. The challenge of psi is bigger than it appears to a race of specialists attempting to "know more and more about less and less." Lindsay and Margenau (**19,** p. 91; **20,** p. 528; **25,** p. 110) have protested against scientific dogmas behind which scientific workers sometimes entrench themselves in order to be able to avoid inconvenient problems. Parapsychology, which is at present in the hands of a straggling minority, is sure to profit by the attentions it is receiving from distinguished physicists like Jordan and Margenau.

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THE NEED FOR RESEARCH WORKERS IN PARAPSYCHOLOGY

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AMONG those who are actively engaged in some phase of investigational work in parapsychology, discussion is likely sooner or later to turn to the fact that there are comparatively few young workers in the field today. Many of the more active investigators are approaching the age of retirement, and the curve of age distribution among workers in the field is unduly weighted on the older end. The situation is the same, too, in more than one country. There is reason, then, for a fairly widespread concern over who is going to carry on.

Perhaps in parts of the world where parapsychology has never been entrenched as an organized program of research, this would not seem so unexpected a situation. For example, since there have been only a report or two of experimental research from Australia, one from Japan, one from India, and only indications of unpublished (however promising) work in South Africa, New Zealand, and Argentina, the lack of actual new growth, or even of continuation of the beginnings already made in those countries, is hardly surprising. Nor should we take too seriously the dearth of reports of well-controlled researches from countries where, in spite of the fact that parapsychology has long been fostered by organized groups, careful experimental methods have never been widely accepted; for example, in Greece, Italy, Austria, and, we could almost (but not quite) add, France. In such countries, no experimental work has been reported that would lead to the expectation of a lively new research program today.

But when we look at those countries where parapsychology has been most actively and competently investigated, as in northwestern Europe and the United States, the problem is inescapable. For the investigations are too largely in the hands of older people. Among the younger ones, in spite of ample interest at the high school, college, and graduate levels, rarely has this interest been sufficiently dominant to lead to continuation of psi investigation even on an avocational basis.

It may help in understanding the situation to consider more closely the different countries involved, observing to what extent the effect varies geographically and looking for suggestions to explain the situation. One of the important questions is whether psi research is making its way (as it eventually must to survive and thrive) into the university laboratory systems. Here, above all, it could best gain new recruits. We might begin with the Scandinavian countries, with their high standard of educational freedom in and out of university life. No publications have come from them that indicate active investigation. The sole exception is the University of Helsinki, where a current research project, not yet published, is being tolerated, if not favored. The only research in the Scandinavian countries that has been pursued in recent years to the point of published report does not come from a university but is the work of a professional engineer, Forwald, in Sweden.

In the Netherlands, university recognition of this field of study is indicated by the professorship of parapsychology held by Tenhaeff at the University of Utrecht. Utrecht was appropriately chosen by the Parapsychology Foundation as the center for the First International Conference of Parapsychological Studies held in 1953. This recognition of parapsychology in a Dutch university might have been expected to accelerate research activities and to draw young researchers into the field. But no such consequence is yet in evidence. In fact, it seems that even in the Netherlands there is a general deficit of young research workers. It needs to be remembered that even though research has been conducted by van Busschbach in the schools of Holland (and this year in North Carolina) and even though Zorab is an active worker, the investigations of these men are not connected with a university. And neither they nor Tenhaeff could be characterized as "younger."

In France the Vasses and Warcollier continue their experimental inquiries quite apart from the very aloof university world of that country. If new experimenters are at work in that country their reports have not yet appeared. In research-minded Germany a little active investigation is carried on here and there. Interest in experimentation is indicated in the fact that a young psychologist, Schweighofer, has translated the Humphrey *Handbook of Tests in Parapsychology* into German for publication in Switzerland. At the University of Bonn a graduate student, Binski, conducted psi experi-

ments in the department of psychology for his Ph.D. thesis research. However, a recent change of professors has taken place. It remains to be seen whether this young worker will be discouraged from undertaking a parapsychological career. Bender, who holds a university professorship at Freiburg, still gives some attention to the field, but as yet we have had no indication that an active research group has developed around him.

Even in Great Britain, historically the most fertile soil for parapsychology, university research is not much encouraged. Fisk, at present its most energetic and productive experimental inquirer, is not university-sponsored. Cambridge has its Perrott Studentship which gives financial aid to an investigator, not necessarily connected with the university (as in the case of Hall, the present holder of the award). Here and there in Britain (or affiliated therewith, like Langdon-Davies in Spain) are other more or less active researches in and out of university communities, some of which may develop into publishable reports. But with rare exceptions, such as that of Roll, a graduate student at Oxford, these special explorations are being made avocationally and mainly by older investigators, more or less established in their own fields (or retired therefrom) and not likely to become the parapsychological research leaders of the future. Thouless, Wiesner, Soal, Goldney, Fisk, and even the newcomer Langdon-Davies, can all be considered senior investigators. One would look in vain for a comparable group of younger men to match them. And so in England, too, future personnel appears to be in short supply.

Has parapsychology, then, been emigrating to the U.S.A.? Unfortunately, the situation in America is not much better. But there are differences. The subject has made at least an entree into college and university circles. But what has been the result in the way of new research personnel? The oldest active university research center is the Parapsychology Laboratory of Duke University. (The earliest, the Stanford Laboratory, has long been extinct.) In the twenty-five year period of existence of the Duke Laboratory an informal course of training has been given to forty or more young people who, at one stage of their careers, were sufficiently interested to wish to do active research in parapsychology. Of these, about one in six has actively continued the investigation of psi problems. Of course, each case is an individual one, and no generalization could

be made that would cover all of them. Some never intended to live a lifetime of research in any field, and marriage made its prior claim on most of the women; death, personality difficulties, and other influences beyond anyone's control came in for their normal toll. But even when all the natural interfering factors are considered, the fact that only a half-dozen (or thereabouts) have persisted in some degree of active research in parapsychology signifies a real problem, the same problem seen in other countries.

The research center at Duke is but one of a number of locations in the U.S.A. where, during the last decade, psi has been more or less continuously under study. Researches have been carried on in a number of other universities, in departments of psychology, and even with the participation in some instances of the psychologists themselves. The work of Schmeidler at The College of the City of New York is the best known of these. Woodruff, of the same college, has worked with Dale of the American Society for Psychical Research. Warner at Claremont Men's College in California has been active from time to time and has encouraged work by students. Nash at St. Joseph's College, Philadelphia, has succeeded in establishing a Parapsychology Laboratory officially acknowledged by the college, and McConnell has established a research center for parapsychology at the University of Pittsburgh, in the Department of Biophysics. All of these have been experimentally productive. But even with this degree of interest in parapsychology in the colleges of America, not very many young career parapsychologists are being produced. Except for the few newer workers at the Duke Laboratory, hardly any of these American researchers mentioned have come into the field within the last ten years. Thus we are evidently dealing with an almost universal situation: a lack of promising new personnel of "first generation" parapsychologists prepared or being prepared to specialize in the field and take over its research program for the future. The field is still left mainly in the hands of "transfer researchers" who crossed over from other areas.

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The very generality of the situation eliminates some of the more easily suggested types of diagnosis. It is obviously to something about parapsychology as a whole and not to any individual or research center or national culture that we must look for an explanation. This larger perspective on the problem is a very helpful one,

especially in making it unnecessary to try to localize the cause. Better still, it can lead one to regard this survey, not as a lamentation, but rather as an analysis of one of the natural difficulties inherent in growth and progress. In fact, the present situation can be seen as a product of normal development; for this shortage of research workers is a symptom of advance, even a significant advance. A generation ago there was not even a sufficiently organized conception of the field of parapsychology to lead anyone to be deeply concerned about future workers. It is certainly progress, then, to have come to the point of feeling the need for trained specialists who will be responsible for the standards and continuity of this new and growing science.

Another related point is that ten or twenty years ago those who were beginning to appreciate the study of psi phenomena as a distinctive branch of science were doubtful as to whether it would be able to establish itself, or maintain itself if established, in the academic institutions of America. The leadership of that day was fighting for the elemental existence of a frontier branch of psychology. Today, while parapsychology is still far from having proper recognition, it is so far on its way that such recognition is plainly just a matter of time. That anxiety is no longer primary and we can afford now to turn to such a question as that of who is going to carry on in the years to come.

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Why, then, are so few experimental scientists being drawn into this field of research? The answer, I think, is connected with the nature of the difficulties confronting the beginner today. It is not a simple matter that can be put into a sentence, but it centers around the combination of frustrations, hardships, and general complications in which a newcomer in parapsychology is now rather sure to find himself.

The field has become more technical. A mere glance over the contents of the twenty years of this *Journal* will bear that out. A student approaching parapsychology today will, for example, more quickly run into statistics. He will find that, although he can go a long way without it, it would be advantageous to have a fair understanding of statistical methods. Also, unless he is already a student of psychology, he will, in the course of his studies, become aware of the need for a general acquaintance with that field. Likewise, because the distinctive and primary concern of parapsychology is

with occurrences manifesting nonphysical properties, he will regret it if he is not somewhat conversant with the general concepts of modern physics. More particularly in recent years, anyone who wishes to move up to the front lines of thinking and research in the field is made to realize that, after all, parapsychology is a branch of biology too. He will greatly appreciate it if he has not neglected biology in his preparatory education.

But most of all, the inquirer will find himself in urgent need of knowing the now considerable literature in the field. Parapsychology is too new to have textbooks, and therefore there is at present no easy way to become acquainted with the background other than to work through the main body of the original literature itself. It will probably take most people some years to do this, and it is safe to say beginners today do not do that type of preparing. In parapsychology there is no formal setup of scheduled reports, examinations, and other measures of progress, and students of this day, even on the graduate level, are not accustomed to such informal self-education.

Rather, the young would-be worker in parapsychology is likely to try to start off without adequate knowledge of the field, hoping to make up what he lacks of this background as he goes. In consequence, he will probably never know what he might have gained from an acquaintance with the mistakes and successes of his predecessors. In any event there is no cadre of instructors available anywhere today to give the time and attention and course work needed for the passing-on of this learning of the past and seeing that no one gets by without it.

It is almost inevitable, then, that the unprepared young man or woman who gets a step or two into the field of active work in parapsychology will be confronted with a difficult situation. It is likely not only to seem confusing but actually to present an exaggerated impression of difficulties which are not, after all, fundamentally much different from the requirements in any branch of research. Other sciences, however, have over a longer time developed methods of selecting newcomers and of educating them for research. For the most part such sciences are sufficiently well established in the universities to have set up required courses that give the student preliminary preparation. No student in those conventional fields would be considered ready to do research until he had

successfully passed through the course of training. This training may, and usually does, require many years (four to six) between the first class and the time for research for an advanced degree.

Compare such a student with the young parapsychologist who tries (and often gets away with it) to conduct experimental inquiries in this new and unfenced field, often within a year of his first cracking of a book on the subject. If he plunges in with unbounded enthusiasm and does not manage to contribute any original research within a year, or at most two, he is likely to consider himself a failure. And there is no dean or other authority to explain to him that he has not given himself or the research a fair opportunity, that the transition is too great in coming into a field from what may be a quite alien area of investigation, or, as often has been the case, without having had any prior experience at all in scientific research. Even worse, his earlier training or experience may have introduced a quite erroneous set of theories and mental habits that need to be lost or corrected before he is fitted to deal with the peculiar and delicate phenomena of parapsychology.

Obviously this is a very frontierish kind of situation. But it is not enough simply to say it is only what is to be expected of a branch of inquiry that is just coming into being as an organized field. It is all right to grant that in an open pioneering area every amateur has a right to travel at his own risk, find out what he can, and make the most of it. After all, every field, no matter how highly organized it is today, had at the beginning no professionals to train its founders; and parapsychology is still somewhat in the founding stage or has so recently emerged that we are seeing the marks and consequences of the groundbreaking period. At such a stage there is no one to do the necessary selecting so that wholly unprepared individuals can be screened out. As it has been, the only test applicable to the personnel that opened up the field was the test of survival, a notoriously harsh one.

Considering what we have a right to expect at this stage of development of the science, there is, in fact, little wonder that there have been as few survivors as there have been—more especially so in view of the very deterrent consideration that none of the rewards for success usually found in a pioneer field of science have been obtainable here, none except the satisfaction that comes from research achievement itself. Rather than professional acclaim and

recognition, there have even been censure and loss of prestige; rather than job opportunity, there has been continued uncertainty as to whether there would even be any foreseeable support for such a branch of inquiry. In short, the would-be experimenter in parapsychology has had to have an exceptionally ardent interest in the possibilities of the field, in the importance of the problems with which it deals, and to be content with the prospect of making a contribution toward their solution. It is expecting a great deal to find many workers both willing and sufficiently qualified to achieve success in the short time allowable, and still ready to commit themselves to making a career of it.

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While we are appreciative of the fact that something has been accomplished and that a few people have met the test and survived the ordeal, we realize that this has happened in spite of the harshly selective approach. It is time now for those who feel some confidence and responsibility in the matter to see what can be done to make the entrance to parapsychology a more accessible one. Some of the improvements most needed can be brought about even though others, equally obvious, would be difficult at present to manage. But even a good look at them will do no harm; they may be more easily handled than they seem.

One of the obvious problems that could be solved is that of developing a method for selecting those who ought to be encouraged to work in parapsychology. It would be too early even yet to try to formulate highly specific requirements for an adequately promising young research worker, but certain safeguarding steps could well be taken that would save a great deal of grief and failure later.

The fairest and safest way to do the selecting would be, somehow, to give first and most help to the best qualified, even while leaving the way open to the individual who, with many handicaps, is still determined to be a parapsychologist. It should be possible, and it would surely be profitable, to coordinate the efforts of those, on the one hand, who are able to encourage young workers with research grants and scholarships, and, on the other, those senior research workers who would be called upon to direct the training and research efforts of these young workers. Such plainly needed coordination could be of enormous value in improving not only the quantity of investigators but their quality as well. If those who

are willing to aid psi investigation, either in its purest laboratory form or in its various possible applications and associations, will make effective use of the guidance of experienced workers in parapsychology, an organized program of scholarships for the training of new experimenters can be developed. Thus the first great difficulty would be removed, for the better qualified candidates would get first consideration.

What is more, such a selective program could, through an advisory council, give much needed direction to the earlier education of potential candidates. Research centers have long been receiving inquiries from students, both in high school and college, asking for advice on the best preparation for a research career in parapsychology. The time has come for something to be done to coordinate such judgments as experienced workers in the field have formed on this matter. The pooling of advice on this and related matters could be managed through the cooperative leadership of the suggested council, and the connection thus established between scholarship grants and training recommendations would be a naturally advantageous one.

Another conspicuous stumbling block in solving this personnel problem has already been mentioned, the difficulty the beginner in parapsychology encounters in getting a general familiarity with the literature of the field. There is need, first of all, for an introductory textbook or handbook on experimental parapsychology, one that would acquaint the student with the general findings and the methods which produced them. As well as assembling the facts generally established, the book should serve as a hallway to the various special sections and subdivisions of the science. It ought to give the beginner, right from the start, an introductory factual perspective with signposts pointing him on to advanced reading. Such a book is now in the course of preparation and, if successful, should serve as a starting base for students (and members of other professions) who would not themselves ever glean the assembled facts from the original sources. With this and other reviews of the field that could now be prepared if the need should be recognized, the parapsychology literature would, like that of other scientific branches, be made available in generalized form for young workers. The immense orientation value of this preparation during the first difficult phase of approach to the research could be decisive.

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Granted that better selection of workers can be made and better educational preparation can be effected than in the past, there is another essential requirement. This is the need for places at which potential research parapsychologists could serve their internships and carry on training in which their ability to become successful experimenters could be tested and developed. Provision could be made as a part of an established program to accept limited numbers of selected and prepared trainees at such research centers for parapsychology as are available. There are, as is well known, too few of these anywhere in the world today and naturally no one center should attempt to manage more than a certain limited number; for such an educational project would demand time and devoted attention from the more experienced; and it may be taken for granted that the senior research worker who would be qualified to instruct and supervise others also would have his own research program and commitments. Moreover, not everyone can be supposed to be both an able and a willing trainer. Accordingly, it would be necessary for research centers to make special provisions for a training program of this kind.

Finally, although by no means at the end of the list of actual needs, comes the important question of professional opportunity. How can it be made possible for more young people to find jobs in which research in parapsychology can be carried out? We must not expect it to make no difference to the rational mind of the candidate whether there is any way to finance a life of research in his chosen field and any possibility of providing for his family.

There is no point in lamenting the fact that billions are annually spent for research on matters that, however deserving, are certainly not so much more important than the question of man's own nature as to justify that they get *all* the available research money. Nor is there anything to be gained except useless bitterness over the current misuse of funds intended by their donors to have been spent in the advancement of parapsychological inquiry. Rather, the practical thing is to understand and face the situation as it is. At present there is no university endowment actually available for scientific research in parapsychology. This will surprise many. In fact, it would not be easy to find any other field of equal importance to mankind of which such a devastatingly discouraging comment could be made.

In the past, most of the research workers in this country began by carrying on research as an incidental part of a university teacher's function. Of course, a recognized part of the university teacher's role is to contribute original research. Usually the teacher has relatively complete freedom in the selection of the topic of his research. Actually, pressure may be exerted in a larger university to prevent an investigation that would conflict with the beliefs and predilections of departmental colleagues. It has been so with parapsychology in some universities in the past. But in many of the smaller institutions there is already a sufficient tolerance and freedom from criticism to enable the individual staff member to pursue research in this field. But younger people can be assured that even in the larger institutions attitudes are changing at an encouraging rate. It may reasonably well be predicted that within another decade parapsychological research will be carried on in many more of the larger universities in this country than at present. The trend is plainly discernible.

Most of those who hope eventually to devote their *full*-scale efforts to research in parapsychology will still have to create their own positions, but that may not be so difficult as it once was. The Parapsychology Laboratory at Duke University is one research center that has maintained itself, at least for several decades. Similar beginnings have been and are being made, and with effort and intelligent planning should continue and be expanded and maintained on into the foreseeable future. And of course the young parapsychologist who hopes for a career in pure research in parapsychology can start off with a teaching position, and "graduate" into full-time research status through the growth of his research program. There is precedent for this!

Another opportunity for the individual desiring full-time university research in the field could be made by organizing supporting groups of people interested in parapsychology. Let the example of the Netherlands in this matter be kept in mind. The Dutch Society for Psychical Research has been supporting the Professorship of Parapsychology at Utrecht. Similar organizations in other countries might do well to try for comparable arrangements to bring the benefits of their endowment to the advantage of research in its best possible environment in our modern culture, the univeristy community.

The Parapsychology Foundation of New York, under the leadership of its president, Eileen J. Garrett, has encouraged parapsychology around the world by making possible a greater degree of international discussion and exchange of ideas, and more especially through its grants-in-aid to research workers. This Foundation, however, like parapsychology itself in the university laboratory, needs the assurance of permanent endowment to give it the kind of confidence in the future which intelligent research planning requires. The time has come when those who recognize the importance of the field will wish to consider the realities of a modern scientific research program. These realities need somewhere to be spelled out fully and plainly. If we want parapsychology to be in the hands of a proper staff of research workers, we must appreciate what the individual investigator faces as he ventures into this naturally hazardous domain of science—a domain that is made more hazardous because of financial insecurity. Men who, for themselves, would think nothing of the risks must often hesitate to impose them on their families.

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It is possible, then, to see that there are improvements that may smooth the potential investigator's approach to the field. In Holland and America parapsychology is apparently making its way into the universities as Professor McDougall, thirty years ago, maintained it should. And in addition, new bonds may be established between parapsychology and adjacent fields of practice or research. Recent studies of ESP in the public schools encourage the thought that it may not be long before trained parapsychologists will be needed in educational research centers, as other specialists have been. The growing interest of psychiatrists in what is taking place in the ESP laboratory has led to definite inquiries about the possibility of research connections between the parapsychological laboratory and the mental hospital. We shall not now go into a discussion of the more practical and utilitarian applications, for there is still the intermediate problem of controlling psi sufficiently for reliable use. We would definitely now, however, not exclude this possibility from consideration although the research field will not likely broaden in that direction until it fills the shorter-range view of commercial or defense needs.

If parapsychology could attract a few able people in the past when its status was still more dubious and its prospects comparatively poorer, it should be able to do much better today. The difficulties we have reviewed are largely removable; corrective action is already being taken and will, we hope, continue. Also, for the really able and stoutly determined young worker, they are not, in any case, insurmountable and never have been. And we cannot wholly regret that the time will be long in coming before anyone will be drawn into parapsychology for quick advancement and easily won fame. But for one who today uses such advantages as there are for determining whether or not he is reasonably well-fitted for the demands of the field, there is enough assurance of success to justify the doing so, especially if he has an adequate appreciation of the significance of the problems.

If the challenge of research in this field is indeed a stiff one, there are those, we believe, who will like it all the better for that. For a long time to come there will not be the competition of numbers that crowd the doorways to all the leading professions of today. But there is this to be said: No one who lives his life through in such a field as ours can ever say with regret in looking backward that a dozen or a hundred others might have done his job as well as he, had he not stood in the way. He can safely say that there was no one else to do whatever it was he was able to accomplish. And so, no one could rightly ask that the course into psi research be made easy and free of risk; but it should be made possible for each one according to his abilities to achieve whatever he can, and eventually for all to enjoy that precious freedom of the true explorer to go his own way in the setting of his research objectives, limited only by the realities of experience and reason and the demands of social existence.

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BOOK REVIEWS

CIBA FOUNDATION SYMPOSIUM ON EXTRASENSORY PERCEPTION. (Edited by G. E. W. Wolstenholme and Elaine C. P. Millar.) Boston: Little, Brown and Co., 1956. Pp. ix + 240. $6.00.

A symposium on extrasensory perception was held in London May 3-5, 1955, under the auspices of the Ciba Foundation. In the interest of informality, attendance was limited to a small group. There were 23 participants of whom 19 were British (one residing in Spain and one in the United States), two American, one French and one Dutch. Nine of the participants are known to the reviewer to have engaged in extensive research in ESP, while two others have written extensively on the statistical or philosophical aspects of ESP research. The Ciba Foundation seems to have been especially interested in the biological aspects of ESP, and a number of the participants were biologists who appear to have been invited because of their knowledge of biological areas in which ESP might conceivably manifest itself, or of animal behavior patterns which might offer a plausible alternative to ESP. Fourteen formal papers were presented by 13 of the participants (S. G. Soal presented two papers). This volume contains the 14 papers and discussions of them, opening remarks by the Chairman, A. S. Parkes of the National Institute for Medical Research, London, and the general discussion which closed the symposium.

Almost every aspect of ESP research was at least touched on, and the most diverse points of view were represented; thus, as might be expected, a fair amount of controversy developed. The volume is more stimulating reading than the usual research report, and it should certainly be read by everyone seriously engaged in ESP research. It would be impossible to do justice in a review to all the topics discussed, and it is inevitable that the reviewer's selection will be somewhat personal and his impressions to some extent subjective.

Following the Chairman's opening remarks, R. A. McConnell began the symposium with a paper on "The Nature of the Laboratory Evidence for Extrasensory Perception." This brief paper presents nothing that will be new to the parapsychologist, and is directed primarily at scientists not well acquainted with the literature of

this subject. It summarizes the existing evidence in an extremely competent fashion. Under the title "The Strength and Weakness of the Available Evidence for Extrasensory Perception," D. J. West of the Society for Psychical Research addresses himself to the apparent paradox that "the majority of scientists retain an attitude of disbelief or indifference" in the face of "well-nigh perfect experimental demonstrations." "To summarize," says Dr. West, "the position regarding the evidence for extrasensory perception appears to be this. Excellently conducted demonstrations have been reported by various workers, but because the conditions which produced good scores are not understood and not reproducible, these experiments are not repeatable in the strict, scientific meaning of repeatability. It is still open to the skeptic to doubt the competence and integrity of those who claim positive results." He further complains that "all the serious work is done by a handful of people who could easily be swamped by cranks. Indeed, the field is so small that any fool can make a name for himself in it. There are no courses of training, no recognized qualifications. Anyone can set up as an authority with the assurance that his reports will be widely boosted in newspapers and popular books. Knowing these facts only too well, parapsychologists tend to look upon each other with a distrust far exceeding the distrust of scientists in general." In the same general field is the paper by J. Fraser Nicol entitled "Some Difficulties in the Way of Scientific Recognition of Extrasensory Perception." He classifies these difficulties under four headings: (1) the seeming irrelevance of the subject to other sciences, (2) failure to produce a repeatable experiment, (3) disagreement on the quality of the printed evidence, and (4) unsubstantiated claims. Under the first two headings, he quite briefly describes the existing situation in terms with which there will be little disagreement. Most of his space is devoted to the third and fourth headings, both of which seem to the reviewer relatively minor obstacles to scientific recognition of ESP. He calls attention to a number of instances in which a particular experiment has been cited with approval by one writer, and has been referred to in lukewarm or derogatory fashion (or not mentioned at all) by another. The reviewer doubts that many of the scientific critics of ESP research have examined the literature so minutely as to have been influenced by such relatively minor discrepancies. Nicol's claim of inconsistencies between statements made in popular

writings and the research reports on which they are presumably based seems to the reviewer to rest on a biased interpretation of the sources quoted. For example, his claim that in the work of Hilton and Baer,[1] control throws gave almost the same average as the experimental throws, seems to have been arrived at by eliminating from consideration the experimental throws with small and large dice (the major part of the total). Moreover, when statements are given as based on unpublished data, one would expect a critic to request such data before undertaking to question the accuracy of the statements on the basis of published data bearing on similar questions.

The reviewer was particularly intrigued by G. D. Wassermann's presentation of "An Outline of a Theory of Organismic Form and Behavior." He claims to have reduced biological phenomena and extrasensory perception to essentially physical terms by extensive use of the "field" concept.

A "field" is a mathematical model which has been found to be extremely useful in the description of many types of quantitative physical relationships. Perhaps some will insist that the field is a physical reality, but this is a question of epistemology which probably will never be settled to everyone's satisfaction. Perhaps the easiest kind of field to describe to the layman is a gravitational field. In the space surrounding a massive body (the earth for example), it is said that a gravitational field exists. A test body (a mass of one gram, for example) placed at any given point of this space will experience a certain attractive force in a particular direction, and both the magnitude and direction of the force vary from point to point. The magnitude of the force which the test body would experience at a particular point of space is called the "field strength" at that point. Thus, we have a mathematical model in which every point of space has a number and a direction associated with it. Essentially this distribution of directed magnitudes in space is the "field." At a point where there is no mass to experience the force, in what sense can the field be said to exist unless the physicist postulates a space-filling ether (as few do nowadays)? Similar remarks would apply to other types of field in which mass is replaced by electric charge or some other physical entity.

Some writers on ESP have emphasized evidence tending to show that it could not be explained by any known kind of physical radia-

[1] "A Comparison of Three Sizes of Dice in PK Tests," by Homer Hilton, Jr., George Baer, and J. B. Rhine *J. Parapsychol.*, Vol. 7 (1943), pp. 172-90.

tion. These arguments have long seemed to the reviewer not very important and somewhat futile. The reviewer has the scientist's faith that when the phenomena of ESP are well understood they will be found to be governed by laws: he doubts, in fact, whether any investigator pictures them to himself as occurring in an entirely capricious and inexplicable fashion. Whatever the laws underlying these phenomena may turn out to be, it seems likely that the field concept may be found useful in describing them, as it has been in so many other lines of research. Wassermann's work is a most valuable start in this direction. Whether it can properly be said that such a field theory reduces ESP to "physical" terms seems to the reviewer largely a matter of definition, and of whether the physicists prove to be more hospitable in the future than the psychologists have been in the past. Wassermann is compelled to postulate a number of new types of field having quite different properties from those now recognized by physics, and it remains for the scientists of the future to decide whether such fields will be considered "physical." At any rate, this theory is likely to be useful and suggestive, and this is more important than whether or not it carries a physical label.

Since his original (and still unfulfilled) promise to publish "statistically significant results, similar to those of psychical research . . . obtainable simply by making selections in published tables of random numbers, as if the tables were themselves the data of a psychical research experiment,"[2] G. Spencer Brown has beat a long retreat, and now says in his brief paper, "The Data of Psychical Research: A Study of Three Hypotheses": "The only thing I have said is that I am not sure that all ESP experiments, or even most of them, are evidence of telepathy and what not." While the reviewer believes that the weight of evidence is against Brown's contentions, he agrees that "statistical significance cannot by itself prove a natural law; at best it is a pointer, signalling where such a law might be found." However, this kind of argument can be pushed only so far. Few will be inclined to attribute the Riess and Pearce-Pratt results cited by McConnell to statistical artifacts; there will be rather general agreement that in these experiments some natural law or laws other than chance were at work. It should be equally apparent that we are now a long way from being able to assert with finality the nature of the laws concerned.

[2] *Nature*, Vol. 172 (1953), p. 154.

S. G. Soal's paper "Some Statistical Aspects of ESP" will only be mentioned here since it is largely based on the book *Modern Experiments in Telepathy* by Dr. Soal and F. Bateman and contains material well known to most readers of the *Journal*. This is true also of his second paper, "A Case of Pseudo-ESP," which describes experiments with the professional mind-reader, Marion. Closely related to this latter paper but broader in scope is E. J. Dingwall's interesting presentation on "The Simulation of Telepathy."

In describing M. Pobers' interesting paper, "Psychical Phenomena Among Primitive Peoples," the reviewer can do no better than to quote in full the author's summary:

"1. Further investigations of telepathic communications among primitive peoples can lead to very significant findings, provided the investigator remembers that for this group of subjects telepathy cannot be a 'game of guessing'; it remains an archaic means of communication in critical and conflictual situations.

"2. We need both laboratory experiments and on-the-spot observations of a wide range of 'bordering phenomena,' and of cases suggesting unusual development of known senses, possibly related to extrasensory perception.

"3. We need a serious and systematic study of psycho-physiological techniques in shamanistic practices.

"4. There are still a few regions in Africa, South America, Asia, and the West Indies where useful work can be done. But the penetration of technological progress is faster than we think. In a few years' time there may be very little left of the phenomena and faculties we are discussing. For the time being, in view of the scarcity of gifted subjects in Europe and in America, investigations in underdeveloped countries offer unusually interesting possibilities."

Experiments with the Spanish subject Maria, described in J. Langdon-Davies' paper, "Extrasensory Perception Among Peasant European Populations," have been the subject of a recent article in the *Journal*.[3]

Probably the most controversial part of the Symposium revolved about the two papers on bird navigation: "The Sensory Nature of Bird Navigation" by G. V. T. Matthews and "Testing for an ESP Factor in Pigeon Homing" by J. G. Pratt. This history of research into the mechanism of pigeon homing has previously been sketched

[3] "ESP Tests with a Spanish Girl," Vol. 19 (Sept. 1955), pp. 155-64.

by Pratt in the *Journal.*[4] It will suffice to say here that a number of theories have been advanced from time to time, some of which have been thoroughly discarded. The only sensory hypothesis under active consideration at present seems to be that of sun-navigation sponsored by Matthews. It seems to be the prevailing opinion among biologists who have worked in this field that the sun-navigation hypothesis has some arguments in its support, but that there are also serious objections which have not been satisfactorily answered. Matthews is here more restrained in his claims than in his book *Bird Navigation;* he concedes that the hypothesis "requires much more testing" and cites the need for a "better appreciation of the precise way in which the bird interprets and acts upon the information provided by the sun." However, the reviewer believes that the time has passed when an interest in ESP can justly be described as "intellectual pimples" and investigation of ESP in animals can properly be discouraged on the ground that the great body of skeptics are not yet convinced of its existence in human beings. Indeed, anecdotal material on homing and "trailing" by domestic pets strongly suggests the possession of such faculties, and there would seem to be little reason to think that man is unique in this respect.

Pratt's work in pigeon homing has been carried out in close collaboration with ornithologists (Gustav Kramer and Ursula von St. Paul) who think, in agreement with most biologists, that the solution to the problem of pigeon homing will ultimately be found within the framework of sensory physiology. It has enjoyed financial support from the office of Naval Research. As exemplified by his paper in the present volume and other research reports, his work represents a carefully reasoned effort to discover the environmental factors which help or hinder the pigeon's homing ability. Much more of this kind of information will certainly be needed before any definitive conclusion as to the homing mechanism can be reached.

In a thought-provoking paper, "Parapsychology in the Modern Approach to Psychosomatic Man," Robert Amadou emphasizes the need to consider parapsychology in its relation to psychology. He stresses that ESP is an activity of the whole man and must be fitted into the framework of his other activities. ". . . if ESP exists at all, how could it be considered as showing itself only in

[4] "The Homing Problem in Pigeons," Vol. 17 (1953), pp. 34-60.

the presence of Zener cards? . . . ESP, if it exists at all, plays a part in every psychical activity, just as do the 'faculties' of memory, will, intelligence, etc. . . . When psychology exists as a true, unique science, parapsychology will exist no longer; but it will have been most useful in bringing out a magnificant result: learning from science all that science can teach us about man."

In the final paper, "Experiences Suggestive of Paranormal Cognition in the Psycho-analytic Situation," W. H. Gillespie calls attention to a receptive attitude toward the existence of psi phenomena on the part of a growing number of psycho-analysts and recounts some interesting personal experiences involving paranormal cognition by patients of his own thoughts and activities.

In the course of the general discussion which closed the symposium, C. G. Butler discussed the special mechanisms employed by bees in communicating the location of food. As the Chairman points out, "such use of faculties which to us seem astonishing, leads one to think that other remarkable faculties may occur in nature and await discovery."

T. N. E. Greville.

Ochéma. By J. J. Poortman. Assen: Van Gorcum & Co., 1954. Pp. 179.

The title of this book, "Ochéma," means carrier or vehicle. In the subtitle, "History and Import of Hylic Pluralism," the author indicates its content, which is a search for an underlying reality that finds expression in all material and spiritual phenomena.

Claiming that dualistic materialism suffers from a selectivity that restricts the study of the problem of the relation of soul and body, the author offers a more comprehensive approach which he calls Hylic Pluralism.

The book consists mainly of a historical review of all forms of philosophic Hylic Pluralism as found in legends and extant literature of all peoples, from the primitive to those of the twentieth century.

Although it presents little of immediate concern to modern parapsychology, it does provide a necessary and extensive background for any philosophical inquiry into the nature of parapsychological phenomena.

Incidentally, those who cannot read the Dutch text can yet gain a clear idea of its content by turning to page 164, where the author has provided, in English, a chapter-by-chapter synopsis.

FREDERICK TEN HOOR

CORRECTION

In the report by Winnifred Nielsen on "Mental States Associated with Success in Precognition" in the preceding issue (June, 1956), two subjects did not complete the experiment and they were therefore not included in the analyses of the mood ratings. Their ESP scores were also omitted in the summary of the results of the precognition tests. Inasmuch as there was no reason why their results should not have been included, their precognition data are reported here as a part of the findings of that experiment.

One of these subjects completed 50 runs with a 0 deviation, and the other, 15 runs with a deviation of −1. When their results are included with those of the other subjects, the totals in Table 1 become: 865 runs, 4535 hits, +210 deviation, and 3.57 critical ratio. Inclusion of these subjects does not affect in any appreciable degree the significance of the data as given in the report.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests a single throw of the dice or other objects thrown.

MEANWHILE, it seems to me probable that in this department of science [parapsychology], as in every other, the wholesome and valuable part of skepticism will ultimately be broken down, if at all, not by any one conclusive experiment, but by converging lines of testimony coming in from many and unexpected quarters; and the breach will be assisted by the gradual perception that such psychical actions as are proved to occur are not portents or ruptures in the order of nature but are natural and simple outgrowths from what science already knows; they are first-fruits from a promised land which has been seen from the hills but has not yet been explored.

It is a most unpardonable blunder for a scientific man to suppose that everything that can be known is already more or less within his cognizance; and his least justifiable attitude is that which holds that there are certain departments of truth in the universe which it is not lawful to investigate.

The same Lord Kelvin who, in a moment of aberration (I hope), wrote this very year that "one-half of hypnotism and clairvoyance is imposture and the rest bad observation," uttered also the worthier sentiment that "Science is bound by the everlasting law of honor to face fearlessly every problem which can fairly be presented to it."

Oliver Lodge, "On the Difficulty of Making Crucial Experiments as to the Source of the Extra or Unusual Intelligence Manifested in Trance-Speech, Automatic Writing, and Other States of Apparent Mental Inactivity," *Proceedings of the Society for Psychical Research*, Vol. X, 1894.